PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVIII.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCVIII.

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PRINTED BY RICHARD TAYLOR,
PRINTER TO THE ROYAL SOCIETY.
MDCCCXCVIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *The* Bakerian *Lecture. Experiments upon the Resistance of Bodies moving in Fluids. By the Rev.* Samuel Vince, *A. M. F. R. S. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

Read November 9, 1797.

In a former Paper upon the Motion of Fluids, I stated the difficulties to which the theory is subject, and showed its insufficiency to determine the time of emptying vessels, even in the most simple cases; I also proved, by actual experiments, that, in many instances, there was no agreement between their results and those deduced from theory. The great difference between the experimental and theoretical conclusions, in most of the cases which respect the times in which vessels empty themselves through pipes, necessarily leads us to suspect the truth of the theory of the action of fluids under all other circumstances. In the doctrine of the resistances of fluids, we see strong reasons to induce us to believe, that the theory cannot generally lead us to any true conclusions. When a body

moves in a fluid, its particles strike the body; and, in our theoretical considerations, after this action, the particles are supposed to produce no further effect, but are conceived to be, as it were, annihilated. But, in fact, this cannot be the case; and what we are to allow for their effect afterwards, is beyond the reach of mere theoretical investigation. Whatever theory therefore we can admit, must be that which is founded upon such experiments as include in them every principle which is subject to any degree of uncertainty. We must therefore have recourse to experiments, in order to establish any conclusions upon which we may afterwards reason. In the paper above mentioned, I described a machine to find the resistances of bodies moving in fluids; since which time, I have made a variety of experiments with it, upon bodies moving both in air and water, and I have every reason to be satisfied of its great accuracy. In this paper, I propose to examine the resistance which arises from the action of non-elastic fluids upon bodies.

This subject divides itself into two parts; we may consider the action of water at rest upon a body moving in it, or we may consider the action of the water in motion upon the body at rest. We will first give the result of our experiments in the former case, and compare them with the conclusions deduced from theory. Now the radius of the axis of the machine made use of in these experiments was 0,2117 in. the area of the four planes was 3,73 in. the distance of their centres of resistance from the axis was 7,57 in. and they moved with a velocity of 0,66 feet in a second. The first column of the following table exhibits the angles at which the planes struck the fluid; the second column shows the resistance by experiment, in the direction of their motion, in Troy ounces; the third column

gives the resistance by theory, assuming the perpendicular resistance to be the same as by experiment; the fourth column shows the power of the sine of the angle to which the resistance is proportional.

Angle.	Experiment.	Theory.	Power.
10°	0,0112	0,0012	1,73
20	0,0364	0,0093	1,73
30	0,0769	0,0290	1,54
40	0,1174	0,0616	1,54
50	0,1552	0,1043	1,51
60	0,1902	0,1476	1,38
70	0,2125	0,1926	1,42
80	0,2237	0,2217	2,41
90	0,2321	0,2321	

The fourth column was thus computed: Let s be the sine of the angle to radius unity, r the resistance at that angle, and suppose r to vary as s^m; then $1^m : s^m :: 0{,}2321 : r$, hence, $s^m = \frac{r}{0{,}2321}$, and consequently $m = \frac{\log. r - \log. 0{,}2321}{\log. s}$; and, by substituting for r and s their several corresponding values, we get the respective values of m, which are the numbers in the fourth column. Now the theory supposes the resistance to vary as the cube of the sine; whereas, the resistance decreases from an angle of 90°, in a less ratio than that, but not as any constant power of the sine, nor as any function of the sine and cosine, that I have yet discovered. Hence, the actual resistance is always greater than that which is deduced from theory, assuming the perpendicular resistance to be the same; the reason of which, in part at least, is, that in our theory we neglect the

whole of that part of the force which, after resolution, acts parallel to the plane; whereas (from the experiments which will be afterwards mentioned), it appears that part of that force acts upon the plane; also, the resistance of the fluid which escapes from the plane, into the surrounding fluid, may probably tend to increase the *actual* resistance above that which the theory gives, in which that consideration does not enter; but, as this latter circumstance affects the resistance at all angles, and we do not know the quantity of effect which it produces, we cannot say how it may affect the *ratio* of the resistances at different angles.

In theory, the resistance perpendicular to the planes is supposed to be equal to the weight of a column of fluid, whose base = 3,73 in. and altitude = the space through which a body must fall to acquire the velocity of 0,66 feet; now that space is 0,08124 in. consequently the weight of the column = 0,1598 Troy oz.; but the actual resistance was found to be = 0,2321 oz. Hence, the actual resistance of the planes : the resistance in our theory :: 0,2321 : 0,1598, which is nearly as 3 : 2.

I am aware that experiments have been made upon the resistances of bodies moving in water, which have agreed with our theory. An extensive set was instituted by D'ALEMBERT, CONDORCET, and BOSSUT, the result of which very nearly coincided with theory, so far as regards the absolute quantity of the perpendicular resistance. Their experiments were made upon floating bodies, drawn upon the fluid by a force acting upon them in a direction parallel to the surface of the fluid. There can be no doubt but that these experiments were very accurately made. The experiments here related were also repeated so often, and with so much care, and the results always

agreed so nearly, that there can be no doubt but that they give the actual resistance to a very considerable degree of accuracy. In our experiments, the planes were immersed at some depth in the fluid; in the other case, the bodies floated on the surface; and I can see no way of accounting for the difference of the resistances, but by supposing that, at the surface of the fluid, the fluid from the end of the body may escape more easily than when the body is immersed below the surface; but this, I confess, appears by no means a satisfactory solution of the difficulty. The resistances of bodies descending in fluids manifestly come under the case of our experiments.

Two semi-globes were next taken, and made to revolve with their flat sides forwards. The diameter of each was 1,1 in. the distance of the centre of resistance from the axis was 6,22 in. and they moved with a velocity of 0,542 feet in a second; and the resistance was found to be 0,08339 oz. by experiment. By theory, the resistance is 0,05496 oz.; hence, the resistance by experiment : the resistance by theory :: 0,08339 : 0,05496, agreeing very well with the abovementioned proportion. But, when the spherical sides moved forwards with the same velocity, the resistance was 0,034 oz. Hence, the resistance on the spherical side of a semi-globe : resistance on its base :: 0,034 : 0,08339; but this is not the proportion of the resistance of a perfect globe to the resistance of a cylinder of the same diameter, moving with the same velocity, because the resistance depends upon the figure of the back part of the body.

I therefore took two cylinders, of the same diameter as the two semi-globes, and of the same weight; and, giving them the same velocity, I found the resistance to be 0,07998 oz.;

therefore the resistance on the flat side of a semi-globe : the resistance of a cylinder of the same diameter, and moving with the same velocity :: 0,08339 : 0,07998. This difference can arise only from the action of the fluid on the back side of the semi-globe, moving with its flat side forwards, being less than that on the back of the cylinder, in consequence of which the semi-globe suffered the greater resistance. The resistance of the cylinders, thus determined directly by experiment, agrees very well with the foregoing experiments. The resistance, *cæteris paribus*, varies as the square of the velocity very nearly, and may be taken so for all practical purposes, as I find by repeated experiments, made both upon air and water, in the manner described in my former paper. Hence, for different planes, the resistance varies as the area × the square of the velocity. Now the resistance of the planes whose area was 3,73 in. moving with a velocity of 0,66 feet in a second, was found to be = 0,2321 oz. Also, the area of the two cylinders was 1,9 in. and their velocity was 0,542 feet in a second; to find, therefore, the resistance of the cylinders from that of the planes, we have $0{,}66^2 \times 3{,}73 : 0{,}542^2 \times 1{,}9 :: 0{,}2321$ oz : 0,07973 oz. for the resistance on the cylinders, differing but a very little from 0,07998 oz. the resistance found from direct experiment.

Now, to get the resistance on a perfect globe, we must consider, that when the back part is spherical, the resistance is greater than when it is flat, in the ratio of 0,08339 : 07998; hence, the resistance on a globe : the resistance on a semi-globe in the same ratio; but the resistance on the semi-globe was 0,034 oz. hence, 0,07998 : 0,08339 :: 0,034 oz. : 0,0354 oz. the resistance of a globe; consequently, the resistance of a globe : the resistance of a cylinder of the same diameter, mov-

ing with the same velocity in water :: 0,0354 : 0,07998 :: 1 : 2,23.

We proceed next to compare the actual resistance of a globe with the resistance assumed in our theory. In the first place, the absolute quantity of resistance has been found to be greater than that which we use in theory, in the ratio of 0,2321 : 0,1598; but, by theory, the resistance of the globe : the resistance of the cylinder :: 1 : 2, or as 1,115 : 2,23; hence, by theory, we make the resistance of the globe too great, in the ratio of 1,115 : 1; and it is too small, from the former consideration, in the ratio of 0,1598 : 0,2321; therefore the actual resistance of the globe : the resistance in theory :: 0,2321 : 0,1598 × 1,115 :: 0,2321 : 0,1782, which is nearly in the ratio of 4 : 3. Thus far we have considered the resistance of bodies moving in a fluid; we come next to consider the action of a fluid in motion upon a body at rest.

A vessel 5 feet high was filled with a fluid, which could be discharged by a stop-cock, in a direction parallel to the horizon. The cock being opened, the curve which the stream described was marked out upon a plane set perpendicular to the horizon; and, by examining this curve, it was found to be a very accurate parabola, the abscissa of which was 13,85 in. and the ordinate was 50 in. hence, the latus rectum was 180,5 in. one-fourth of which is 45,1 in. which is the space through which a body must fall to acquire the velocity of projection; hence, that velocity was 189,6 in. in a second. And here, by the by, we may take notice of a remarkable circumstance. The depth of the cock below the surface of the fluid was 45,1 in. hence, the velocity of projection was that which a body acquires in falling through a space equal to the whole depth of the fluid; whereas,

through a simple orifice, the velocity would have been that which is acquired in falling through half the depth; the pipe of the stop-cock therefore increased the velocity of the fluid in the ratio of $1 : \sqrt{2}$, and gave it the greatest velocity possible; the length of the pipe was 3 in. and the area of the section 0,045 in.; also, the base of the vessel was a square, the side of which was 12 inches.

The area of the section of the pipe may be found very accurately, in the following manner. The vessel being kept constantly full, receive the quantity of fluid run out in any time t'', and then weigh it, by which we shall be able to get the quantity in cubic inches. Now if $v =$ the velocity of the fluid when it issues from the pipe, $a =$ the area of the section of the pipe, $l =$ the length of the cylinder of water run out, whose base $= a$, and $m =$ the quantity of fluid discharged in t''; then $v : l :: 1'' : t''$, hence, $l = vt$; but $al = m$; therefore $avt = m$; hence, $a = \frac{m}{vt}$. In the present instance, $t = 20$, $m = 170{,}63$ cubic inches, $v = 189{,}6$; hence, $a = 0{,}045$.

Let A B C D (fig. 1. Tab. I.) be a solid piece of wood, upon which are fixed two upright pieces, rs, tu; between these, a flat lever eac is suspended, in a perpendicular position, on the axis xy, and nicely balanced; and let a be a point directly against the middle of the axis, in a line perpendicular to the plane of the lever. This apparatus is placed against the stop-cock, at the distance of about 1 inch, and, when the water is let go, let us suppose the centre of the stream to strike the lever perpendicularly at e; take $ac = ae$, and, on the opposite side to that at which the stream acts, fasten a fine silk string at c, and bring it over a pulley p, and adjust it in a direction perpendicular to the plane of the lever, and, at the end which hangs

down, fix a scale Q, the weight of which is to be previously determined. All the apparatus being thus adjusted, open the stop-cock, and let the fluid strike the lever, and put such weight into the scale as will just keep the lever in its perpendicular situation, and that weight, with the weight of the scale, must be just equivalent to the action of the fluid. Thus we get the perpendicular effect of the water. Now incline the plane of the lever, at any angle, to the direction of the stream, and adjust the string perpendicular to the plane, as before; then put such a weight into the scale as will keep the lever perpendicular to the horizon, whilst the fluid acts upon it, and you get that part of the effect of the fluid which acts perpendicular to the plane. In this manner, when the fluid acts oblique to the plane, we get the perpendicular part of the force. The second column of the following table shows this effect, by experiment, for every 10th degree of inclination shown in the first column; and the third column shows the effect, by theory, from the perpendicular force, supposing it to vary as the sine of inclination.

Angle.	Experiment.			Theory.		
	oz.	dwts.	grs.	oz.	dwts.	grs.
90°	1	17	12	1	17	12
80	1	17	0	1	16	22
70	1	15	12	1	15	6
60	1	12	12	1	12	11
50	1	18	10	1	18	17
40	1	4	10	1	4	2
30	0	18	18	0	18	18
20	0	12	12	0	12	19
10	0	6	4	0	6	12

It appears from hence, that the resistance varies as the sine of the angle at which the fluid strikes the plane; the difference between the theory and experiment being only such as may be supposed to arise from the want of accuracy to which the experiments must necessarily be subject.

Let us now first consider, what the whole perpendicular resistance by experiment is, when compared with that by theory. Now, by theory, the resistance is equal to the weight of a column of the fluid, whose base = 0,045 in. and altitude = 45,1 in. and the weight of that column is = 1 oz. 1 dwt. 10 grs. Hence, the resistance by theory : the resistance by experiment :: 1 oz. 1 dwt. 10 grs. : 1 oz. 17 dwts. 12 grs. :: 514 : 900.

In the next place, let us examine what is this resistance, compared with the resistance of a plane moving in a fluid. We here prove, that the resistance of the fluid in motion acting on the plane at rest : the resistance by theory :: 900 : 514; and we have before proved, that the resistance by theory : the resistance of a plane body moving in a fluid :: 1598 : 2321; hence, the resistance of a fluid in motion upon a plane at rest : the resistance of the same plane, moving with the same velocity, in a fluid at rest :: 900 × 1598 : 514 × 2321 :: 1438200 : 1192954 :: 6 : 5 nearly. Now we know that the actual effect on the plane must be the same in both cases; and the difference, I conceive, can arise only from the action of the fluid behind the body, in the latter case, there being no effect of this kind in the former case. For, in respect to the pressure before the body, that will probably be the same in both cases; for there is a pressure of the column of the spouting fluid, acting against

the particles which strike the body at rest, similar to the action of the fluid before the body, upon the particles which strike the body moving in the fluid. Hence, the resistance of the planes moving in the fluid, with the velocity here given, is diminished about one fifth part of the whole, by the pressure behind the body; but, with different velocities, this diminution must increase as the velocity increases.

The effect of that part of the force which acts *perpendicular* to the plane being thus established, we proceed next to examine, what part of the whole force which acts *parallel* to the plane, is effective. To determine which, the axis *w v* (fig. 2.) was fixed perpendicular to the plane of the lever *a b c d*, and the ends of the axis were conical, and laid in conical holes; and the thread from which the scale was hung was fixed to the edge at *e*, and acted perpendicular to it and the weight drew the lever in the direction *e s*, contrary to that in which the fluid tends to move the lever, and it acted at the same perpendicular distance from the axis below, as the fluid acted above it. Let *x m z* be a line parallel to the horizon, when the lever is perpendicular to it, and which passes through the centre of the stream; and let *x m z* be also the direction of that part of the force which acts parallel to the plane. This apparatus being adjusted, the experiments were made for every tenth degree of inclination; and here a circumstance took place, for which I can give no satisfactory reason. Having gone through the experiments once, and noted the results, I repeated them; and, to my great surprise, I found all the second results to be very different from the first. The experiments were therefore repeated again, and the results were still different. Being certain that the experiments were very accurately made each time, I

was totally at a loss to conjecture to what circumstance this difference of results was owing. By repeating however the experiments, and observing at what point of the line $x\,m\,z$ the centre of the stream acted, I discovered that the effect varied by varying that point; that it was greatest when the stream struck the lever as near as it could to x; less when it struck it at the middle m; and least when it struck it as near as it could to z, notwithstanding the stream acted at the same perpendicular distance from the axis in each case, and the parallel part of the force always acted in the line $x\,m\,z$. At the angles 80°, 70°, 60°, the fluid striking as near as it could to the edge z, gave the lever a motion, not in the direction $x\,m\,z$, but in the opposite direction $z\,m\,x$, as appeared by taking away the scale. I have therefore marked such results with the sign ——, the motion produced being then in a direction opposite to that which ought to have been produced, by that part of the force of the stream which acts parallel to the plane of the lever. The forces which are here put down, are those which take effect in a direction parallel to the plane of the lever, for every tenth degree of inclination; the perpendicular force being 1 oz. 17 dwts. 12 grs.

		dwts.	grs.
At 80° incl.	Edge z	——	
	Middle m	3	3
	Edge x	10	17
At 70° incl.	Edge z	——	
	Middle m	6	2
	Edge x	11	10
At 60° incl.	Edge z	——	
	Middle m	7	9
	Edge x	11	22

		dwts.	grs.
At 50° incl.	Edge *z*	0	17
	Middle *m*	8	20
	Edge *x*	13	21
At 40° incl.	Edge *z*	1	16
	Middle *m*	8	6
	Edge *x*	13	15
At 30° incl.	Edge *z*	3	20
	Middle *m*	7	2
	Edge *x*	12	15
At 20° incl.	Edge *z*	4	16
	Middle *m*	6	0
	Edge *x*	11	12
At 10° incl.	Middle *m*	5	12

It is a remarkable circumstance, that the effect of the fluid at *z* increased regularly as the angle decreased; for, though I did not measure the negative effects, I could plainly perceive that that was the case; whereas, the effects at *m* and *x* increased to about the middle of the quadrant, and then decreased. At 10°, the obliquity was such, that the section of the stream extended very nearly from one side of the lever to the other.

As it appears by experiment, that the velocity of the fluid flowing out of the vessel was equal to the velocity which a body acquires in falling down the altitude of the fluid above the orifice, the square of the velocity must be in proportion to that altitude. To find therefore, in this case, whether the resistance varied as the square of the velocity, I let the water flow per-

pendicularly against the plane (fig. 1.) at different depths, and I always found the resistances to be in proportion to the depths, and therefore in proportion to the square of the velocity, agreeing with what takes place when the body moves in the fluid.

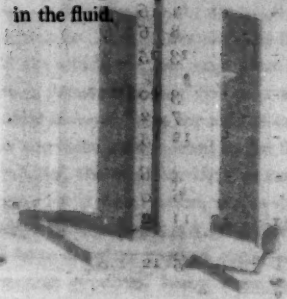


Fig. 1.

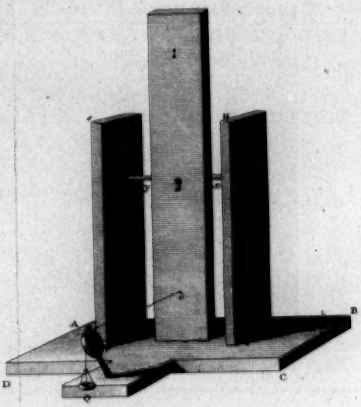
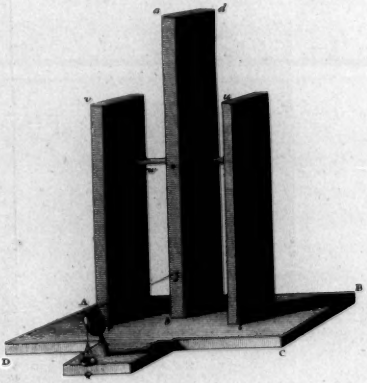


Fig. 2.



II. *Experiments and Observations, tending to show the Composition and Properties of Urinary Concretions.* By George Pearson, *M. D. F. R. S.*

Read December 14, 1797.

I. *Historical Observations.*

URINARY concretions have obtained their denominations, like most other things, from their obvious properties. Accordingly, in our language, they are popularly known by the names Stone and Gravel, or Sand, from their resemblance to the states of earth so named: and we find names of the same import in other languages, such as λιθος, (ARETÆUS;) λιθιασις, (CÆLIUS AURELIANUS;) ψαμμος, (ARETÆUS;) λιθδια, (various authors;) *Calculus*, (CELSUS and PLINY;) *Sabulum*, (various authors.) In other languages, and especially in those now spoken, it is unnecessary to notice names which have the same meaning.

The notion very generally entertained, of the nature of urinary concretions, consisted with the terms, till the last twenty years; although the experiments of SLARE, FREDERIC HOFFMAN, and HALES, long before showed that these substances commonly consist of animal matter. GALEN indeed imagined that φλεγμα, or viscid animal matter, is the basis of animal concretions; but, in his days, earth was believed to be the basis of animal matter. Alkaline medicines were, however, employed by the Greek physicians, in diseases from calculi.

The experiments of the alchemists also made it appear, that earth was only a part of the matter of concretions. It was probably the observation of the deposition and crystallization of saline bodies, which suggested the notion of urinary calculi being of the nature of tartar. Such was the opinion of BASIL VALENTINE, and after him of HOCHENER, better known by the name of PARACELSUS; but, whether the latter adopted the denomination *Duelech* from its import, or from caprice, has not been explained. VAN HELMONT, a century after his prototype PARACELSUS, being struck with the experiment in which he discovered the concretion of salts in distilled urine by alcohol, was led to depart from his adored master's opinion, with respect to the nature of calculi; although he acknowledges the merit of PARACELSUS, in having discovered the solvent *Ludus*, (a calcareous stone also called *Septarium*,) which VAN HELMONT says is preferable to alkaline lixivium. He also says, that when the archeus spirit of urine meets with a volatile earthy spirit, and does not act in a due manner, a concretion will be formed; but, in a healthy state, although all urine contains the matter of urinary calculi, no concretion can take place, because the archeus, or vital power of the bladder, counteracts its formation.

As to the kind of earth composing calculi, the only distinction of earths, till about the last half century, was into absorbent and non-absorbent; but, since the absorbent earths were distinguished into calcareous, magnesia, and alumine or clay, the calcareous was considered to be the earth of urinary concretions; apparently however for no other reason but its obvious properties, and its extensive diffusion through the whole animal kingdom.

At length, *viz.* in 1776, the experiments of the wonderful SCHEELE were published in Sweden, but were scarcely known in this country till 1785. These experiments exploded the opinion of the earthy nature of calculi, and substituted that of their consisting of a peculiar acid, resembling the succinic, and of a gelatinous matter, without any earth. Afterwards about $\frac{1}{200}$ of their weight of lime was found by BERGMAN; which, for a cause now well known, had eluded the acuteness of SCHEELE. Although the experiments of SCHEELE were confessedly unquestionable, and were ably supported by the learned BERGMAN, some very eminent chemists, having obtained different results by their own experiments, adopted a different opinion of the composition of these concretions. The immortal, and ever to be deplored, LAVOISIER supposed these substances to consist of acidulous phosphate of lime and animal matter, many of them being partially fusible; but still it was the unrivalled SCHEELE who discovered, that the urine of healthy persons contains superphosphate, or acidulous phosphate, of lime; and who also indicated the experiment which verified his opinion, that phosphate of lime is the basis of bone.

Experiments have been likewise made, for the most part in a rather desultory way, and most of them by persons but little practiced in chemical inquiries, which at least afford evidence, that urinary concretions are very different, with respect to the proportion of the ingredients in their composition, and perhaps also in kind. M. FOURCROY, who however must not be classed with inexperienced chemists, I believe first obtained prussic acid by fire, and by nitric acid, from these concretions; and showed that they sometimes contain phosphate of ammoniac and of soda; which may be dissolved out of them by

water. M. FOURCROY also says, he found magnesia in the intestinal calculus of a horse; which calculus was a triple combination, of one part of phosphate of ammoniac, two parts of magnesia, and one of water, besides traces of animal and vegetable matter.

Dr. LINK, in a very elaborate dissertation, published at Gottingen, in 1788, on urine and calculi, concludes that urinary concretions consist of phosphoric acid, lime, ammoniac, oil, the bases of different kinds of gazes, together with the acid sublimate of SCHEELE, although he did not succeed in obtaining it.

It is a proof of Dr. BLACK's sagacity, that he should have been able to perceive, from a few experiments, that urinary concretions consisted of animal matter and the earth of bone, before the composition of this earth was demonstrated by GAHN.

In this historical sketch it should be noticed, that alkaline substances, though used by the Greek physicians, and afterwards by the alchemical physicians, appear to have been laid aside by the regular practitioners, for a century or two preceding their revival, by the famous Mrs. STEPHENS, in 1720. Her prescription brought into vogue the theory of these medicines operating by their causticity. The successful use, by Mr. COLBORNE, of potash saturated with carbonic acid, according to the discovery of BEWLEY and BERGMAN, and the still further improvement in practice, from the use of soda, as well as potash, super-saturated with carbonic acid, by the discovery of a peculiar method by Mr. SCHWEPPE, have completely refuted the theory of the agency of alkalies on the principle of causticity.

It appears, from the preceding brief history, as well as from the confession of the latest and best writers, that the experi-

ments hitherto made, rather "afford indications of what re-"mains to be done, than furnish demonstrations of the nature "of animal concretions." It is also too obvious to need explanation, that more efficacious and innocent practice, in diseases from these concretions, can only be discovered by a further investigation of their properties. It is with this view, as well as for the sake of chemical philosophy, that I think it my duty to submit to the Society some of the observations I have made, in the course of inquiry on this subject.

The observations which I shall now offer, are principally on a substance, which my experiments inform me is very generally a constituent of both urinary and arthritic concretions. It is a substance obtained by dissolving it out of these concretions, by lye of caustic fixed alkali, and precipitating it from the solution by acids. In this way, SCHEELE separated this matter; but he did not consider its importance, nor of course at all investigate its properties. He does not even seem to have been aware that it was a distinct constituent part of the urinary concretion; for, when he relates the experiment of precipitating matter from the nitric solution of calculus by metallic salts, no distinction is made between the precipitations in this experiment, and that in the former; yet we can now show, that in the one case the precipitate is a peculiar animal oxide, and in the other they are metallic phosphates. As SCHEELE obtained an acid sublimate, it has been imagined by some writers, that the precipitate by any acid (even by the carbonic) from the alkaline menstruum, was an acid; the same as that obtained by sublimation, and which, in the new system of chemistry, has been denominated *lithic acid*. The following experiments show that these substances are different species of matter.

II. EXPERIMENTS.

250 grains of a white, smooth, laminated, urinary calculus, and the same quantity of a nut-brown one, with an uneven surface, both of which were of a roundish figure, were pulverized together.* 300 grains of these pulverized calculi were triturated with three ounces and a half, by measure, or five ounces, by weight, of lye of caustic soda. The mixture became thick, and copiously emitted ammoniacal gaz. After digestion for a night, and then boiling, with the addition of five ounces of pure water, I obtained, by filtration, five ounces of clear colourless liquid. Boiling water was repeatedly poured upon the strainer, till what passed through it was almost tasteless, and remained clear, on the addition of diluted sulphuric acid.

(*a*) The matter remaining on the strainer, being dried, was an impalpable, white, tasteless, heavy powder, which weighed 96 grains.

(*b*) The five ounces of filtrated liquid, having been set apart, on standing, deposited a white, opaque, granulated, soap-like matter, from a colourless clear liquid. The liquid being decanted, the deposit was dried, and was then an opaque, brittle, soap-like matter, which dissolved readily in water, giving a clear but not viscid solution, and tasting weakly of soda. This soap-like matter weighed 280 grains.

(*c*) The decanted liquor, (*b*,) being mixed with the above filtrated liquors, on evaporation to three ounces, afforded no deposit on standing, although it was a very heavy and soapy

* The object of these experiments being principally to investigate the properties of one of the constituent parts of urinary concretions, which part was previously determined (by the test of nitric acid,) to exist in both these, it can be no objection to the experiments, that I made use of a mixture of two calculi.

liquid to the feel; but, on adding diluted sulphuric acid gradually, till it ceased to become turbid, a sediment was deposited, which was a very light, white, impalpable powder, in weight, when dried, 26 grains. The liquid from which this powder was precipitated, being evaporated, afforded nothing but sulphate of soda, and a few grains of crystals, which seemed to be phosphate of soda. There was also a blackish matter, which burnt like horn, or other animal matter, and did not leave a pink or rose-coloured matter, on evaporating the solution of it in nitric acid to dryness, but left a carbonaceous residue; whereas, the white precipitate, so treated, afforded a beautiful pink matter.

(*d*) 250 grains of the soap-like matter (*b*) being dissolved in eight ounces of pure water;

1. A little of this solution, further diluted by one ounce of water, grew milky on adding a few drops of nitric acid, but became less so on standing. On adding more nitric acid, and heating it, the mixture became quite clear: by adding a few drops of lye of caustic soda, a very slight curdy appearance took place.

2. On adding, to the same diluted solution, a little of the diluted sulphuric or muriatic acid, milkiness ensued, and remained, although the acids were added till the mixture was extremely sour. On adding lye of caustic soda, much more than to saturate the superabundant acid, the mixture became clear again; and, on adding the acids a second time, the milkiness was reproduced. It was found that the milkiness could be produced and destroyed, or clearness be produced, by the alternate addition of the acid and alkali, for an unlimited number of times. If the nitric acid however was used, at length

no milkiness could be induced. If carbonate of soda was added, in place of the caustic soda, the mixture could not be made clear.

3. Lime water was rendered turbid by this solution, but I neglected to examine the precipitated matter.

4. A little of the solution, with the addition of a few drops of concentrated nitric acid, being evaporated to dryness, sometimes a pink, and at other times a blood-red, or rose-coloured matter was left; which, by further application of fire, became black.

5. Carbonic acid, digested and shook with this solution, did not render it turbid.

6. To the whole of the remaining solution was added diluted sulphuric acid, to saturate the alkali. On standing, a copious precipitate took place, from a clear liquid; which precipitate, being washed and dried, was a mass of very light, mica-like, whitish crystals, amounting to 123 grains. It was estimated that the solution used in the Experiments 1.—5. would have produced 12 grains, and that the 30 grains of soap-like matter, (*b*,) not decompounded, would have yielded about 14 grains more.

(*e*) The precipitate, (*d*, 6.)

1. Had no taste, nor smell, and did not dissolve in the mouth.

2. About one part of it only dissolved in 800 parts of boiling water; which solution did not redden paper stained with turnsole, nor the solution and tincture of this test; neither did it change turnsole paper, reddened by acid, to a blue colour. On cooling, the greatest part of what had been dissolved was deposited, in a crystallized state, equally on the sides and bottom of the vessel. This crystallized matter had the properties abovementioned (*d*.). Boiling water was found to dissolve a much greater proportion of *urinary stone*, and also of *gravel*, than of this precipitate.

3. Lye of mild potash, or subcarbonate of potash, being dropped into the solution (*e*, 2.) with its crystallized deposit, the crystals at first seemed to dissolve; but, on standing, a great part of the matter was deposited, and the liquid remained turbid.

4. The precipitate being boiled with lye of carbonate of soda, more seemed to be dissolved than in pure water; but the solution was not clear, and, on evaporating it nearly to dryness, and pouring cold water upon it, on a paper strainer, scarcely any thing but the soda passed through with the water; the precipitate remaining behind on the paper. The result was the same, when this experiment was made with a lye of carbonate of ammoniac. The result was also the same, with water in which red oxide of mercury had been boiled; which was also boiled with this precipitate, and filtrated after cooling.

5. A little of the precipitate being triturated with quicklime, hot water was poured upon it. The filtrated liquor gave the precipitate back again, on adding muriatic acid.

6. The precipitate exposed to flame, with the blowpipe, turned black, emitted the smell of burning animal matter, and evaporated or burnt away without any signs of fusion; staining the platina spoon black.

7. Five grains of the precipitate, in half an ounce of water, were left to stand in a warm room, during the months of August and September last, without any signs of putrefaction appearing, or any obvious change taking place.

8. Twenty-four ounces of boiling water were saturated with the precipitate, and divided into six portions; from each of which, on cooling, most of it again precipitated.

The first portion, on boiling with a little lye of carbonate of

soda, (the pneumatic apparatus being affixed,) discharged no carbonic acid into lime water; but a transparent solution was produced, and, on cooling, very little was precipitated.

The second portion was, in the same manner, boiled in a little lye of caustic soda; which gave a transparent solution on cooling, without any precipitation.

The third portion being boiled with lime water, very little more seemed to be dissolved than in pure water.

The fourth portion being boiled with 4 grains of subphosphate of lime, or calcined bone, no more seemed to be dissolved on account of this addition.

Nor was more dissolved in the fifth portion, by the addition of 4 grains of phosphate of lime, made by dropping phosphoric acid into lime water.

And the result was the same with the sixth portion, to which were added 4 grains of superphosphate of lime, made by adding phosphoric acid to lime water, so as just to make a clear solution, and then evaporating the solution.

9. Urine seemed to dissolve, or at least to suspend, a greater quantity of the precipitate than mere water; so likewise did water with a little sulphate of soda.

10. The precipitate did not render solution of hard soap at all curdy; but, on adding the precipitate to solution of sulphuret of potash, it became very turbid.

11. The precipitate produced a strong effervescence, even in the cold, with nitric acid, but the fumes were not those of nitrous acid: there was a clear solution, which, on evaporation to dryness, afforded black matter, surrounded by a pink, or blood-red margin.

12. The substance, with sulphuric acid, turned black, and

emitted fumes copiously, which were scarcely those of sulphureous acid; and, on evaporation, a black mark only was left.

13. I first digested, and then boiled, in water, the precipitate with prussiate of iron; but the filtrated liquor afforded no precipitation with sulphate of iron.

14. Two drachms, by measure, of nitric acid, of the specific gravity of 1,35, were poured upon 7 grains of the precipitate. A violent effervescence took place, which was soon succeeded by a complete solution.

A few drops of this solution, being evaporated on glass, left a black mark, surrounded by a pink margin. A few drops of nitric acid being evaporated from this residue, nothing but a still less black mark, and a few red spots remained.

Nitric acid being added a third time, nothing but a black mark, still smaller, remained; which entirely disappeared, on evaporating this acid from it a fourth time.

I found that a few drops of this solution, so diluted that they did not contain the $\frac{1}{400}$, or even a much smaller part, of a grain of the precipitate, on evaporation, left a pink stain on glass.

The whole of the rest of the solution was distilled in a very low temperature, so that a drop only fell about every half-minute, till a thick brownish sediment remained, with a red margin. A similar distillation was performed, with the distilled liquor, a second time, when there remained a little whitish thick matter. On a third distillation, as before, with the distilled liquor, towards the close white fumes arose, and about half a drachm of liquid, which now remained in the retort, being left to stand, prismatical crystals, decussating each other, were formed. They had a sharp taste, but were scarcely sour;

were very soluble in the mouth, and evaporated in white fumes, leaving a very slight black stain.

15. Twenty grains of the precipitate were introduced into a tube, $\frac{1}{3}$ of an inch wide in the bore, sealed by melting at one extremity; which extremity was coated, and the tube was fitly bent for retaining sublimate, and collecting gaz. The temperature, from the fire applied, was at first very low, but was gradually increased, so as to make the coated part, containing the charge, red hot. At first, the precipitate turned black, and a little water appeared. Secondly, gaz came over, which had the smell of empyreumatic *liquor cornu cervi*. Thirdly, a brown sublimate appeared, and gaz as before, but also with prussic acid gaz. Fourthly, black matter, staining the tube, as if from tar, or animal oil. On cooling, there was found a residue, of nearly three grains, of pure carbon. The sublimate was principally carbonate of ammoniac; the rest was animal oil. The gaz discharged was nearly half its bulk, or 5 cubic inches by measure, carbonic acid; and the remaining 5 cubic inches were nitrogen gaz, containing prussic acid and empyreumatic oil.

I treated in the same manner, the same quantity of reddish crystals, deposited spontaneously from urine. The result was not very different from that of the former experiment. The gaz was more offensive, smelling like putrid urine, and the carbonaceous residue was more copious, and contained lime and phosphoric acid; at least the lixivium of it became white, on dropping into it oxalic acid; and it became slightly curdy, on adding lime water.

I treated in the same manner, some quite round and smooth

concretions, of the size of black pepper seeds. The products were the same as the former, but the gaz was still more offensive, and in smaller quantity; and the carbonaceous matter was more copious.

I, in the same way, subjected to experiment 20 grains of a nut-brown light calculus, which I had previously ascertained to contain the matter above described, which was precipitated from caustic soda by acids. The products were of the same kind as the former; but I could find no trace of phosphoric acid in the residue, which I did of lime, and the gaz was less offensive. The carbonaceous residue was not, in weight, 3 grains.

It will be proper, before I proceed further, to point out some of the more obvious conclusions from the above experiments.

1. It appears that at least one half of the matter of the urinary concretions subjected to the above experiments united to caustic soda, and was precipitated from it by acids. (II. *a—d.*)

2. This precipitate does not indicate acidity to the most delicate tests; (*e*, 2.) and, as it is inodorous, tasteless, (*e*, 1.) scarcely soluble in cold water, (*e*, 2.) does not unite to the alkali of carbonate of potash, of soda, or of ammoniac, (*e*, 3, 4.) nor to oxide of mercury, (*e*, 4.) nor to the lime of lime water, (*e*, 8.) nor decompound soap, (*e*, 10.) or prussiate of iron, (*e*, 13.) and, as its combination with caustic soda resembles soap, more than any double salt known to consist of an acid and alkali, this precipitate does not belong to the genus *acids*.

3. As this precipitate could not be sublimed, without being decompounded, like animal matter, (*e*, 15.) and also for the

reasons mentioned in the last paragraph, it cannot be the same thing as the *acid sublimate* of SCHEELE, or the succinic acid.

4. As it does not appear to be putrescible, nor form a viscid solution with water, it cannot be referred to the *animal mucilages*.

5. On account of its manner of burning in the air, under the blowpipe, (*e*, 6.) and its yielding, on exposure to fire in close vessels, the distinguishing products of animal matter, (especially ammoniac and prussic acid,) as well as on account of its affording a soap-like matter with caustic soda, this precipitate may be considered as a species of animal matter; and, from its composition being analogous to that of the substances called, in the new system of chemistry, *animal oxides*, it belongs to that genus. Its peculiar and specific distinguishing properties are, *imputrescibility, facility of crystallization, insolubility in cold water*, and, that most remarkable property of all others, *producing a pink or red matter, on evaporation of its solution in nitric acid.**

I do not avail myself of various other conclusions in this place, because they relate especially to the agency of medicines for preventing and removing concretions; and of course do not properly fall within the views of the Royal Society.

Having found the above precipitate to be an oxide, and not, as is commonly supposed, an acid, I thought it probable that,

* It is much to be wished that we possessed equally delicate tests of the other species of animal matter, which are confounded together, although, from their obvious properties, there is reason to believe they are of very different kinds, as is the case with the matter of the brain, liver, voluntary muscles, mucus, &c. Mr. HUNTER has discovered a distinguishing specific property of pus, and one is here indicated for the oxide of urinary concretions.

like other analogous oxides, it was *acidifiable*, and I suspected that I had really rendered it into the acid state, by the nitric acid; which, in the above experiments, (*e*, 14.) had imparted oxygen to it, and thereby rendered it soluble, deliquescent, pungent, and volatile. This change also would account for the nitric solution not affording the precipitate.

In order to obtain, for examination, an adequate quantity of this supposed acid, the following experiments were instituted, with the three acids (*viz.* the oxymuriatic, the nitro-muriatic, and the nitric,) which can acidify oxides analogous to the present one.

Experiment 1. Twenty-five grains of the above animal oxide, (for so I will now venture to call it,) and three ounces of nitric acid, of the specific gravity of 1,25, were put into a retort, and the hydro-pneumatic apparatus was adjoined.

At a very low temperature, a clear solution was made. First, soon after the solution began to boil, 23 ounces, by measure, of colourless gaz came over, which were succeeded (secondly,) by white fumes, filling the apparatus, and 23 ounces more of gaz. Thirdly, a white sublimate ascended, and there was a strong smell of prussic acid. The sublimate was very readily washed out, being very soluble, and tasted pungent or sharp, but not sour. Fourthly, the distillation being renewed, more white sublimate appeared, but only 3 ounces more of gaz came over; and then the retort only contained a dark-brown solid matter.

The first portion of gaz, *viz.* 23 ounces, consisted of about equal bulks of carbonic acid and atmospherical air. The second portion, *viz.* 23 ounces, was two-thirds of its bulk carbonic acid, and the rest nitrogen gaz. The third portion,

or 3 ounces, was atmospherical air, with a little carbonic acid.

Nitric acid was poured, in the same quantity as before, into the retort. An effervescence immediately took place, which was succeeded by a transparent solution. The distillation yielded gaz of the same kind as before, but in smaller quantity, with white fumes, and white sublimate. When only about 4 drachms, by measure, of liquid remained in the retort, a little of it was evaporated; and, when reduced to a solid matter, it turned black, and took fire, leaving a carbonaceous residue; but, before this, a margin of beautiful pink matter appeared.

Nitric acid was poured, as before, into the retort, for the third time, but very little gaz ascended, and much less white fumes than before. The distillation proceeded, till about one drachm-measure of liquid remained in the retort: this being left to stand, prismatic crystals were formed in a very small quantity of liquid. These crystals did not taste sour, but sharp, and they reddened turnsole-paper. Adding a little soda to a part of them, to see whether I could form a neutral salt, I was surprised by the extrication of ammoniac. To another portion of crystals I added sulphuric acid, which disengaged nitric acid. A third portion of crystals, being exposed over a lamp, wholly evaporated, without leaving a mark behind. The remaining matter in the retort being examined, was found to be nitrate of ammoniac. It was plain that the nitric acid had, by parting with oxygen to the carbon of the oxide, formed carbonic acid. The carbon being thus carried off, of course the nitrogen and hydrogen of the oxide uniting produce ammoniac; which, uniting with the redundant nitric acid, composes nitrate of ammoniac; but great part of the nitrate of ammoniac was carried off in the

vapour state, exhibiting white fumes, and sublimate, as above observed.

The mode of making the experiments with the other acids was of course different from the former experiment.

Experiment II. Twenty-five grains of the above animal oxide, and half an ounce of water, were put into a bottle capable of containing three pints; a stream of oxymuriatic acid gaz, from manganese and muriatic acid, was made to pass into the bottle, and upon the charge, for twelve hours; and, for twenty-four hours more, oxymuriatic gaz kept issuing, but in smaller quantity, and circulating through the bottle. The oxide, by this time, was completely dissolved. Upon adding lime to a little of the solution of it, ammoniac was disengaged; and, upon adding sulphuric acid, there was a disengagement of oxymuriatic acid. On evaporation, however, I obtained nothing but muriate of ammoniac, with which was mixed a little manganese.

In this experiment, I could not doubt that the carbon had been carried off, in the state of carbonic acid, by the oxygen of the oxymuriatic acid; and thus ammoniac was compounded, from the union of the two remaining constituent parts of the oxide, *viz.* the nitrogen and hydrogen. The oxymuriatic acid, united to the ammoniac, parted with oxygen, and became muriatic acid during evaporation; hence, muriate of ammoniac was formed.

Experiment III. The above experiment was repeated, only the gaz was nitro-muriatic gaz, from a mixture of nitric and muriatic acids. The result was the same as in the last experiment, except that the product was a mixture of nitrate, and muriate, of ammoniac.

I made other experiments of the same kind, but their results were so nearly the same as those above related, that I shall not give an account of them. By the unexpected issue of these experiments, all my hopes of acidifying the animal oxide were exploded; but I am indebted to that pursuit, for the curious discovery of the change of the most common basis of urinary concretions, (the animal oxide,) into ammoniac and carbonic acid, by the oxygen of the above acids; which will be found extremely important, as it enables us to interpret many phænomena, in a variety of cases beside the present. It now appears, that the inflammation mentioned in one of the above experiments, (and which also happened in several others,) on evaporation of the nitric solution of the animal oxide, was from the nitrate of ammoniac, the *nitrum flammans* of the old chemists, compounded in those experiments. This inflammation takes place sometimes, on evaporation of nitric solutions, both of urinary concretions, and of urine itself evaporated to the state of soft extract, on account of the ammoniac already existing in these substances. The composition of ammoniac also explains the disappearance of the whole matter of some sorts of urinary concretions, a very small residue of black matter excepted, by repeated affusion and evaporation of nitric acid, from the solution of them in this menstruum.

It remains for me to give an account of the 96 grains of powdery matter left on the paper strainer, (*a*;) which are the insoluble portion, in lye of caustic soda, of 300 grains of urinary concretions.

1. A small portion of the insoluble matter, being exposed to flame with the blowpipe, did not turn black, nor yield any

smell of animal matter; but it became whiter, and I could just agglutinate the powder into one mass, although I was unable to render it fluid.

2. The filtrated liquid, from a little of the matter boiled in water, became very turbid and white with oxalic acid: with lime water it grew barely curdy; and it did not alter the colour of turnsole, or of violet juice.

3. The matter dissolved completely in muriatic acid, and also in nitric acid, without effervescence.

This nitric solution, having been evaporated, to carry off most of the free acid, instantly became very curdy on the addition of lime water.

It grew thick and white on adding sulphuric acid, yielding a copious precipitate of sulphate of lime. One portion of the supernatant liquor upon this precipitate, on evaporation, afforded an extract-like matter; which readily melted, as phosphoric acid does when it is mixed with a little earthy matter. To the other portion of this supernatant liquor was added liquid caustic ammoniac, producing a precipitate which afforded no sulphate of magnesia with sulphuric acid.

From these experiments it appears, that the above 96 grains of insoluble matter consisted of phosphate of lime. Accordingly, the 300 grains of urinary concretions examined, appear to contain,

	grains.
Peculiar animal oxide - - - -	175
Phosphate of lime - - - -	96
Ammoniac, (and most probably phosphoric acid united to the ammoniac,) water, and common mucilage of urine, which were not collected and weighed, by estimation	29
	300

I shall next relate some experiments, made in order to obtain the acid sublimate of SCHEELE, or lithic acid of the new system of chemistry.

100 grains of an urinary concretion, which had been previously found to contain principally the above animal oxide, were introduced into a tube ¼ of an inch wide; which was sealed at one end by fusion, and which also was fitly bent for collecting sublimate, and obtaining gaz. The sealed end was coated and exposed to fire, first to a low temperature, and gradually to a very elevated one.

1. Gaz was discharged, which had the smell of burning bone.

2. Water appeared boiling immediately over the charge, which seemed to be burning, and was turned black.

3. Gaz was discharged, of the smell of empyreumatic *liquor cornu cervi*, and about half a drachm of this liquor was in the upper part of the tube.

4. A brown sublimate of carbonate of ammoniac appeared in the cold part of the tube; but in the hotter part, near the charge, was tar-like matter, and the gaz discharged had a very offensive smell of empyreumatic animal oil, with which was mixed that of prussic acid.

The coated part of the tube was kept red hot, for some time after gaz ceased to come over.

The quantity of gaz amounted to 24 ounces, by measure: it consisted of nearly 16 ounces of carbonic acid gaz, and the rest was air, with a larger proportion of nitrogen gaz than is contained in atmospheric air.

5. There was a residue of 30 grains, almost pure carbon; and 10 grains of heavy black and brown matter, a little above the coated part of the tube. In this last mentioned matter

were many small white *spicula*. At about half an inch above the carbonaceous residue, dark gray matter had been raised, which weighed 15 grains.

This sublimed gray matter did not contain any ammoniac, nor throw down any prussiate of iron, with sulphate of iron. It reddened turnsole paper and tincture. It dissolved in caustic soda; from which solution muriatic acid precipitated nothing; for, although on dropping it into the solution milkiness appeared, the liquid soon grew clear again.

Ten grains of this sublimate dissolved in four ounces of boiling water; which being evaporated to half an ounce, there was, on cooling, a copious deposit of white *spicula*.* The sublimate had a sharp, but not sour taste. Being boiled in muriatic acid, and also in nitric, it did not dissolve at all; but remained, on evaporation to dryness, in the same state as before; and it must be particularly observed, that it left no red or pink matter, on evaporating the nitric acid from it. Sulphuric acid did not act upon it in the cold; but, when heated, it dissolved it, without effervescence, from which solution nothing was precipitated by caustic soda; on evaporating it to dryness, black fumes arose, leaving behind only a black stain. This sublimed matter did not render lime water turbid. Boiled in muriatic acid, so as to carry off all but a very little free acid, on the addition of lime water there was no turbid appearance, but milkiness ensued on adding oxalic acid.

The *spicula*, in the 10 grains of sublimate above mentioned, seemed to be of the same nature as the matter just described.

* From the deposition of these *spicula* by cooling, and from many of the following properties, they appear to be analogous to benzoic acid.

The whole of this sublimate amounted, by estimation, to 18 grains; and I apprehend it is the acid sublimate of SCHEELE.

The sublimate of carbonate of ammoniac amounted to 20 grains; and it was black empyreumatic animal oil which stained the tube.

This experiment was repeated, on 120 grains of a nut-brown, very light, urinary concretion. The result was not very different from that of the former experiment, except that the gaz contained a portion of hydrogen gaz. There were 30 grains of the above described *spicula*, principally mixed with carbonaceous matter: they were light, and had only a very slight sharp and bitter taste.

The experiment repeated a third time, with 80 grains of urinary concretion, afforded 15 grains of the white *spicula* above described, mixed with carbonaceous matter. These I found did dissolve in a large proportion of muriatic acid; which solution yielded them, on evaporation, in the same state as before. Under the flame applied by the blowpipe, they first melted, and then evaporated, without any smell; leaving a slight black mark. Turnsole was reddened by these *spicula*.

In a fourth experiment, I found the white *spicula* contained in the carbonaceous matter united, on boiling, with carbonate of soda, as well as with caustic soda; but, as before, muriatic acid precipitated nothing from the solution. These *spicula* could not be dissolved in nitric acid; nor did the solution of them in water become turbid with oxalic acid. Their taste was, as before, rather bitter and sharp than sour. A very suffocating smell issued forth, on breaking the tube used in this experiment, but it was not from sulphur, nor from prussic acid.

These experiments afford evidence of the wide difference between the animal oxide above described and the acid sublimate of SCHEELE.*

If this conclusion be allowed to be just, it will be necessary to give a name to this urinary animal oxide. Agreeably to the principles of the new chemical nomenclature, the name should be *Lithic oxide*. But the term *lithic* is a gross solecism; and I trust that philological critics will find the name *ouric* or *uric oxide* perfectly appropriate; for, if it be thought objectionable, on account of the existence of the matter in arthritic as well as urinary concretions, still philology will allow its admission, as in other similar cases, κατ' ἐξοχην; it being found in greater abundance, by far, in the urinary passages than in other situations, and therefore falling under common observation, as an ingredient of the urine. If, however, the term lithic oxide, or any other denomination, shall obtain acceptance, I shall very willingly adopt it.

It requires no sagacity, in a person acquainted with the facts of the preceding experiments, to perceive that they are applicable to a variety of uses in chemical investigation, and in the practice of physic. The latter I of course take no notice of in this place; but, relative to the former uses, I shall particularly point out, that we are now able not only to detect, in the easiest manner, the *presence* of the minutest proportion of the above animal oxide in urinary concretions, and also in other substances, but even to determine its *proportion* to the other constituent parts,

* From these experiments, it now appears very doubtful whether the *lithic acid* of SCHEELE exists as a constituent of urinary concretions, or is compounded, in consequence of a new arrangement taking place, of the elementary matters of the concretion, by the agency of fire; but it is demonstrated, that the urinary animal oxide is really a constituent part, and even a principal one, of almost all human urinary calculi.

in the space of a few minutes, in most cases, and in all in a very little time, without any other apparatus than nitric acid, a round bottomed matrass or glass dish, and a lamp. By this method, I have, in a general way, examined above 300 specimens of concretions, of the human subject and other animals, principally urinary ones; and also many from other parts, particularly those from the joints. For these opportunities I am beholden to several professional gentlemen; whose willingness to furnish me with specimens, I shall have much satisfaction in acknowledging on a future occasion. At present, I must acknowledge my obligations to Mr. HEAVISIDE, in whose museum I found between 700 and 800 specimens. The liberal possessor of this treasure offered me, what I could not have taken the liberty of requesting, namely, permission to break off pieces from any of the articles, for experiment. Mr. EDWARD HOWARD did me the honour to take upon himself the task of writing down the reports, and otherwise assisted me.

At this time I shall only mention,

1. That out of 200 specimens of urinary calculi, not more than six did not contain the animal oxide above described, *i. e.* about 32 out of 33 contained it.

2. That the proportion of this oxide was very different; varying from $\frac{1}{100}$ (exclusive of water,) to $\frac{122}{100}$; but, for the most part, varying between $\frac{10}{100}$ and $\frac{140}{100}$.*

3. That the common animal mucilage of urine is frequently found in concretions, in very different proportions; but is perhaps never a principal constituent part of them.

* In some urinary concretions, the interior part contained this oxide, and the exterior part had none of it. On the contrary, in other urinary concretions, the exterior part contained it, and the interior part did not.

4. That the above animal oxide was not found in the urinary concretions, or any other concretions, of any animal but the human kind.

5. That this animal oxide was found also in human arthritic calculi, but not in those of the teeth, stomach, intestines, lungs, brain, *&c.*

P. S. I think proper to subjoin a few experiments, made after the preceding paper was written, which afford evidence of the truth of some of my conclusions, and enable us to explain several properties of animal concretions.

I. *On an Urinary Concretion from a Dog.*

This calculus may be said to be a great curiosity, for it is probably the only specimen in London. I owe the opportunity of examining it to Mr. H. Leigh Thomas, who met with it in the course of his dissections; and therefore we have unquestionable authority, that the concretion was really from the urinary bladder of a dog. It is worthy to be noticed, that the animal appeared to be in perfect health.

This concretion is of an oval figure; is three inches and three quarters in length, and three inches in breadth; is white as chalk; its surface is rough and uneven. Being sawed through longitudinally, no nucleus was found, nor was it laminated, but near the centre it was radiated, and contained shining *spicula*. In other parts it was, for the most part, compact and uniform in its texture. It weighed nearly ten ounces and a half. Its specific gravity was found to be greater than that of human urinary concretions, in general; which I have learned by experiments is also the case with urinary and intestinal

concretions of other brute animals, especially with those of the horse.

The specific gravity of the present calculus was 1,7.

That of one from the urinary bladder of the human subject, of the sort called mulberry calculus, and which consisted almost entirely of uric oxide, was 1,609.

That of another human urinary concretion, of the same composition as the former, but quite smooth, extracted by Mr. FORD, was 1,571.

1. The present calculus of the dog had no taste, nor smell, till exposed to fire.

2. Under the blowpipe it first became black, and emitted the smell of common animal matter; it next smelt strongly of empyreumatic *liquor cornu cervi*; and, after burning some time, became inodorous, and white, and readily melted, like superphosphate of lime.

3. On trituration with lye of caustic soda, there was a copious discharge of ammoniac.

4. It dissolved, on boiling in nitric acid: the solution was clear and colourless; and, on evaporation to dryness, left a residue of *white bitter matter*, which, under the blowpipe emitted, weakly, the smell of animal matter.

5. Upon distilling a mixture of 150 grains of this concretion pulverized and two pints and a half of pure water, to three ounces, the distilled liquid was found to contain nothing but a little ammoniac. The three ounces of residuary liquid, being filtrated and evaporated, yielded 20 grains of phosphate of ammoniac, with a little animal matter; and the residuary undissolved matter amounted to 67 grains.

6. These 67 grains, being triturated with four ounces of caustic

soda lye, discharged very little ammoniac. On distilling this mixture to one ounce, a very small proportion only of ammoniac was found in the distilled liquid. The residuary ounce of alkaline liquid was filtrated, and mixed with the water of elutriation of the undissolved matter. One half of those liquids, on evaporation to dryness, afforded a dark brown matter, amounting to 20 grains, which consisted of phosphate of lime and animal matter. To the other half of the alkaline liquids was gradually added muriatic acid, which occasioned a deposit, in small proportion, of matter that dissolved in nitric acid, but which, on evaporation to dryness, left behind only a brownish matter, consisting of phosphate of lime and animal matter.

7. The residuary insoluble substance in caustic lye, (6.) under the blowpipe, first turned black, and then grew white, but could not be melted.

By diluted sulphuric acid it was decompounded. On the addition of nitrate of mercury, to the filtrated liquid, it yielded phosphate of mercury; and, with oxalic acid, it afforded oxalate of lime; but no sulphate of magnesia was found remaining after these precipitations were produced.

These experiments fully demonstrate, that the above concretion of a dog contained none of the uric or lithic oxide above described, but that it consisted, principally at least, of phosphate of lime, phosphate of ammoniac, and animal matter.

The present instance leads me to explain the reason of the fusibility of calculi. This is demonstrated, by the above experiments, to depend upon the discharge and decomposition of the ammoniac of the phosphate of ammoniac, during the burning away of the animal matter; hence the residuary phosphoric

acid readily fuses, and, uniting to the phosphate of lime, composes superphosphate of lime, a very fusible substance.

The phosphate of ammoniac being dissolved out by water, or caustic alkaline lye, the remaining matter is infusible, being phosphate of lime.

A very hard, brittle, and blackish intestinal calculus of a dog, from Mr. WILSON, was found to be of greater specific gravity than human urinary calculi, and to have the same composition as that of the dog above described.

This also was found to be the composition of a white, smooth, round, intestinal calculus of a horse, the specific gravity of which was 1,791.

The same composition was discovered, on examining a very hard, gray, brittle, laminated, quadrilateral concretion, said to be from the urinary bladder, but which, I think, was more probably from the intestines, of a horse.

II. *On a Calculus from the urinary Bladder of a Rabbit.*

This is also a curiosity, being the only instance I have seen. I am likewise indebted to Mr. THOMAS for this specimen, which he very kindly sent me, fitted up as a preparation, included in the bladder itself. Mr. THOMAS found this concretion, on dissecting a perfectly healthy and very fat rabbit.

This specimen is spherical, and of the size of a small nutmeg. It is of a dark brown colour, has a smooth surface, is hard, brittle, and heavy. When broken, it appeared to consist of concentric laminæ. Its specific gravity was 2.

1. Under the blowpipe it grew black, and emitted the smell of animal matter while burning; at last it ceased to emit any

smell; and, urged with the intensest fire, showed no signs of fusibility.

2. It readily dissolved, with effervescence, like marble, in both muriatic and nitric acids, giving clear solutions.

3. The nitric solution (2.) being evaporated partly to dryness, and partly to the consistence of extract, the dry residuary matter was white; and the extract-like matter, which was bitter, could not be fused under the blowpipe; but, when brought to the state of a powder, the particles of it were made to cohere loosely together into one mass.

4. On dropping sulphuric acid into the muriatic solution, (2.) turbidness, and a copious white precipitation, immediately ensued, from the composition of sulphate of lime.

From these experiments it is warrantable to conclude, that the above urinary calculus of a rabbit consisted principally of carbonate of lime and common animal matter, with, perhaps, a very small proportion of phosphoric acid: it certainly contained no uric oxide.

I examined, in the same manner, a concretion which was said to be from the stomach of a monkey; but I have not evidence of its origin equally satisfactory as that of the two last calculi. Its composition was found to be similar to that of the calculus of the rabbit, *viz.* carbonate of lime and animal matter. Its obvious properties were also the same; it was of the size of the largest nutmeg.

III. *On urinary Concretions of the Horse.*

I examined several specimens in cabinets, said to be vesical calculi of the horse, and found none of them to contain the uric oxide above described; but that they consisted (as well

as the calculi from the stomach and intestines of the same animal) of phosphate of lime, phosphate of ammoniac, and common animal matter, which melted like superphosphate of lime, after burning away the animal matter and ammoniac. As these, and some other experiments, seemed to concur in establishing an important truth, I thought it necessary to examine an urinary concretion of a horse, which, from its figure and size, was unquestionably from the kidney of that animal; for I have found by experience, that one cannot depend entirely on the accounts in cabinets, nor indeed, sometimes, on the assertions of persons who collect specimens.

1. This concretion, which Dr. BAILLIE was so good as to give me, was of a blackish colour, was very brittle and hard, and had no smell or taste. It felt heavier than human urinary calculi.

2. Under the blowpipe it became quite black, and emitted the smell, weakly, of common animal matter. It was reduced very little in quantity, and showed no appearances of fusibility, after being exposed for a considerable time to the most intense fire of the blowpipe.

3. Muriatic acid dissolved this concretion, with effervescence, yielding a clear solution; which, on evaporation to dryness, left a black and bitter residue.

4. A little of the residue (3.) being boiled in pure water, to the filtrated liquor superoxalate of potash was added; which occasioned a very turbid appearance, and copious white precipitation.

5. Nitric acid also readily dissolved this concretion, with effervescence. The solution being evaporated, partly to dryness, and partly to the consistence of an extract, the dry residuary

matter was white and bitterish, and the extract-like part showed no signs of fusibility under the intensest fire of the blowpipe.

6. A little of the concretion, being triturated with lye of caustic soda, emitted no smell of ammoniac.

From these experiments it appears, that this calculus, like the former one from a rabbit, consists of carbonate of lime and common animal matter.

A renal calculus of a horse, in Mr. HEAVISIDE's collection, appeared, on examination, to consist of carbonate of lime and common animal matter.

Another specimen, however, of renal calculus of a horse, in the same collection, marked No. 3. was found to consist of phosphate of lime, phosphate of ammoniac, and common animal matter. It was fused under the blowpipe.

The specimen marked No. 8. in the same collection, which was said to be a vesical calculus of a horse, appeared to consist of the three ingredients just mentioned.

I have met with two instances of a deposit of a prodigious quantity of matter in the urinary bladder of horses, which had not crystallized, or even concreted: it amounted, in one specimen, which was given to me by Dr. MARSHALL, to several pounds weight; and in the other, which is in the possession of Mr. HOME, to about 45 pounds. Its composition was, principally, carbonate of lime and common animal matter. *

I have not found any instance of human urinary calculi of a

* Since this paper was read, Mr. BLIZARD has been so attentive as to send me another specimen of the same kind of deposit as those here mentioned. It now appears probable, that such deposits frequently take place, although I believe they have not been noticed before.

similar composition to that of the rabbit, and those of horses above described, which consist of carbonate of lime and animal matter; and I believe that human urinary calculi very rarely occur of a similar composition to those of the dog and horses above mentioned, which were found to consist of phosphate of ammoniac, phosphate of lime, and animal matter, without containing *uric oxide*.

The difference in the constitution of urinary concretions may depend on the difference of the urinary organs of different animals, on the food and drink,* and on the various diseased and healthy states of the urinary organs.

I have not found the uric oxide in the urinary concretions of any phytivorous animal; but, whether it would be formed in the human animal when nourished merely by vegetable matter, must be determined by future observations. In the mean time, it is warrantable to conclude, from analogy, that it would not, and the application of this fact to practice is obvious; but I now purposely avoid making any practical inferences, until I can, at the same time, state a number of facts I have collected, relative both to concretions and to the urine itself.

* I found the stomach-concretion called *Oriental Bezoar*, to consist merely of vegetable matter; as did the intestinal concretion of a sheep.

III. *On the Discovery of four additional Satellites of the* Georgium Sidus. *The retrograde Motion of its old Satellites announced; and the Cause of their Disappearance at certain Distances from the Planet explained.* *By* William Herschel, *LL.D. F.R.S.*

Read December 14, 1797.

HAVING been lately much engaged in improving my tables for calculating the places of the Georgian satellites, I found it necessary to recompute all my observations of them. In looking over the whole series, from the year of the first discovery of the satellites in 1787 to the present time, I found these observations so extensive, especially with regard to a miscellaneous branch of them, that I resolved to make this latter part the subject of a strict examination.

The observations I allude to relate to the discovery of four additional satellites: to surmises of a large and a small ring, at rectangles to each other: to the light and size of the satellites: and to their disappearance at certain distances from the planet.

In this undertaking, I was much assisted by a set of short and easy theorems I had laid down for calculating all the particulars respecting the motions of satellites; such as, finding the longitude of the satellite from the angle of position, or the position from the longitude: the inclination of the orbit from the angle of position and longitude: the apogee: the greatest

elongation; and other particulars. Having moreover calculated tables for reduction: for the position of the point of greatest elongation; and for the distance of the apogee, or opening of the ellipsis; and also contrived an expeditious application of the globe for checking computations of this sort, I found many former intricacies vanish.

By the help of these tables and theorems, I could examine the miscellaneous observations relating to additional satellites, on a supposition that their orbits were in the same plane with the two already known, and that the direction of their motion was also the same with that of the latter.

And here I take an opportunity to announce, that the motion of the Georgian satellites is retrograde.

This seems to be a remarkable instance of the great variety that takes place among the movements of the heavenly bodies. Hitherto, all the planets and satellites of the solar system have been found to direct their course according to the order of the signs: even the diurnal or rotatory motions, not only of the primary planets, but also of the sun, and six of their secondaries or satellites, now are known to follow the same direction; but here we have two considerable celestial bodies completing their revolutions in a retrograde order.

I return to the examination of the miscellaneous observations, the result of which has been of considerable importance, and will be contained in this paper. The existence of four additional satellites of our new planet will be proved. The observations which tend to ascertain the existence of rings not appearing to be satisfactorily supported, it will be proper that surmises of them should either be given up, as ill founded, or at least reserved till superior instruments can be provided, to

throw more light upon the subject. A remarkable phænomenon, of the vanishing of the satellites, will be shewn to take place, and its cause animadverted upon.

I shall now, in the first place, relate the observations on which these conclusions must rest for support, and afterwards join some short arguments, to shew that my results are fairly deduced from them.

For the sake of perspicuity, I shall arrange the observations under three different heads; and begin with those which relate to the discovery of additional satellites.

A great number of observations on supposed satellites, that were afterwards found to be stars, or of which it could not be ascertained whether they were stars or satellites, for want of clear weather, will only be related. For, to enter into the particular manner of recording these supposed satellites, or to give the figures which were delineated to point them out, would take up too much time, and be of no considerable service to our present argument. It ought however to be mentioned, that nearly the same precaution was taken with all the related observations as, it will be found, was used in those that are given in the words of the journals that contain them. The former will be distinguished under the head *Reports*, the latter under that of *Observations*.

Investigation of additional Satellites.

Reports.

Feb. 6, 1782. A very faint star was pointed out as probably a satellite, but Feb. 7 and 8 was found remaining in its former situation.

March 4, 1783. A satellite was suspected, but March 8 was found to be a star.

April 5, 1783. A suspected satellite was delineated, but the 6th it was seen remaining in its former place.

Nov. 19, 1783. A supposed satellite was marked down, but no opportunity could be had to account for it afterwards.

Nov. 16, 1784. Supposed 1st and 2d satellites were pointed out, but not accounted for afterwards.

Many other fruitless endeavours for the discovery of satellites were made; but, finding my instrument, in the NEWTONIAN form, not adequate to the undertaking, the pursuit was partly relinquished. The additional light however which I gained, by introducing the Front-view in my telescope, soon after gave me an opportunity of resuming it with more success.

Jan. 11, 1787. Three supposed satellites were observed: a first, a second, and a third. Jan. 12, the 1st and 2d were gone from the places in which I had marked them, but the 3d was remaining, and therefore was a fixed star.*

Jan. 14. A supposed 3d satellite was delineated, but on the 17th it was found to be a star.

Jan. 17. Supposed 3d, 4th, and 5th satellites were marked, but were found remaining in their former places on the 18th.

Jan. 24. Supposed 3d and 4th satellites were noted, but the weather proving bad on the succeeding nights, till February 4, they were lost in uncertainty.

Feb. 4. A 3d satellite was marked, but not being afterwards accounted for remains lost.

Feb. 7. A supposed 3d satellite was proved to be a star the 9th.

Feb. 10. Supposed 3d and 4th satellites have not been afterwards accounted for.

* It has already been shewn, in a former paper, that the removed satellites were those two which now are sufficiently known.

Feb. 13. Supposed 3d, 4th, and 5th satellites proved stars the 16th.

Feb. 16. A 3d satellite proved a star the 17th.

Feb. 19. Supposed 3d and 4th satellites were proved to be stars the same evening, by being left in their places, while the planet was moving on.

Feb. 22. The supposed 3d and 4th of the 19th were seen remaining in their former places; and new 3d, 4th, and 5th satellites were marked; but these were lost through bad weather, which lasted till March 4.

March 5. A supposed 3d satellite proved to be a star the 7th.

March 7. The position of a 3d was taken, and a 4th also marked; but March 8 they were both proved to be fixed stars.

October 20. A very small star was seen near the planet, but lost, for want of opportunity to account for it.

March 13, 1789. The positions of 3d and 4th satellites were taken, but the 14th they were found to be stars.

March 16. Supposed 3d and 4th satellites were well laid down, but March 20 were found to be stars.

March 26. The places of supposed 3d and 4th satellites were ascertained, but no opportunity could be had of deciding whether they were stars or satellites.

Dec. 15. A supposed 3d satellite was accurately delineated, but proved to be a star the 16th.

Observations.

" Jan. 18, 1790. $6^h\ 51'$.* A supposed 3d satellite is about

* All the times have been corrected so as to be true, sidereal; but are only given here to the nearest minute.

"2 diameters of the planet following; excessively faint, and "only seen by glimpses."

"7^h 57′. I cannot perceive the 3d."

Reports.

Jan. 18, 1790. A supposed 4th satellite was described, but was found to be a star the 19th.

Jan. 20. A 3d satellite was perceived, and its angle of position ascertained; but was afterwards lost, for want of opportunity to examine its place again.

Observations.

"Feb. 9, 1790. 6^h 28′. There is a supposed 3d satellite, in a "line with the planet and the 2d satellite."

"6^h 40′. Configuration of the Georgian planet and satel-"lites." See Tab. II. fig. 1.

"Clouds prevent further observations."

"Feb. 11. The supposed 3d satellite of the 9th of February "I believe is wanting; at least I cannot see it, though the wea-"ther is very clear, but windy."

"Feb. 12. The supposed 3d satellite of the 9th is not in "the place where I saw it that night."

Reports.

Feb. 11, 1790. Supposed 3d and 4th satellites were laid down, but on the 12th they were both found remaining in their former places.

Feb. 16. A 3d satellite was delineated, but on the 17th it proved to be a star.

March 5. Supposed 3d and 4th satellites were laid down, but on the 8th were seen remaining in their places.

Feb. 4, 1791. A 3d satellite was marked, but has not been accounted for afterwards.

Feb. 5. Supposed 3d, 4th, and 5th satellites were delineated, but no opportunity could afterwards be found to ascertain their existence.

March 5. Supposed 3d, 4th, and 5th satellites were put down. They could not be seen March 6, but were proved to be small stars the 7th.

Feb. 12, 1792. A third satellite was delineated, but was left behind by the planet the same evening, and also seen in its former place the next night.

Feb. 13. A 3d satellite was put down, but proved to be a star the 14th.

Feb. 20. The position of a 3d satellite was taken, but 4 hours after was found to be left behind by the planet. It was also seen in its former place Feb. 21.

Feb. 26. A 3d satellite, between the planet and 2d, was observed; which, 3^h 37′ afterwards, was thought to be left behind, but was so faint as hardly to be perceivable. A fourth was also put down. Neither of them have been accounted for afterwards.

March 8, 1793. The position of a supposed 3d satellite was taken, but the next day it was found to be a star.

March 9. A supposed 3d satellite was observed, at 5 or 6 times the distance of the 1st, but was not accounted for afterwards.

March 14. Supposed 3d and 4th satellites were observed, but no opportunity could be had afterwards to see them again.

Observations.

"Feb. 25, 1794. With 320, there is a small star *a*, fig. 2. "about 15 degrees north preceding the planet; and another *b*, "about 30 degrees north preceding: also one *c*, directly pre- "ceding. There is a very small fourth star *d*, making a trape- "zium with the other three; and two more *e f*, preceding this "4th star, are in a line with it."

"Feb. 26. The stars, in figure 2, marked *f e d a*, are in a line. "There is a star *g*, at rectangles to *f e d a*: the perpendicular "falls upon *d*: it is towards the south. There is also a star *b*, "north of *f e d a*; but it is too faint to admit of a determination "of its place: I can only see it now and then by imperfect "glimpses."

"Feb. 28. 6^h 40′. The stars *f e d a* of the 26th are in their "places. *c* is in the place where I have marked it. The star "*g* is in the place where I marked it. I see also the very "small star *b*."

"6^h 50′. There is a very small star *k*, but not so small as *b*, "very near to, and north following *f*, which I did not see on "the 26th. It is not quite half way between *f* and *e*, but "nearer to *f* than to *e*. It makes an obtuse triangle with *f* "and *e*."

"9^h 48′. The motion of the planet this evening, since the "first observation, is very visible."

"10^h 7′. I cannot perceive the star *k*. The weather is not "so clear as it was."

"10^h 21′. I cannot perceive the star *k* in the place where it "was 6^h 50′."

"March 4, 1794. Power 320. 6^h 46′. The stars *a b c d e f g*

" of Feb. 28, fig. 3. are in their places, but I cannot see the " small star *k*. The evening is not very clear."

" 9h 51'. I cannot see the star *k*."

" 10h 25'. I suppose *a*, in figure 4, to be the star towards " which the planet is moving."

" *c a b* are in a crooked line.

" *c e f* are nearly in a line; *f* is a little preceding.

" *c d e* form a triangle.

" There is a small star *b*, preceding *d*.

" There is an exceeding small star *k*, in the line *b k g*, but a " little preceding and nearer *b*.

" *a b c* are large stars.

" *d e g* are also pretty large.

" *f* and *b* are small. Power 157.

" With 320, there is also a very small star *l*, near *d*, forming " an isosceles triangle *b d l*, on the preceding side."

" March 5. 7h 39'. Power 320. The stars *a b c d e f g b k l* " are in the places where they were marked last night."

" 9h 37'. There is a very small star *n*, south of *g*; another *m*, " preceding *g*; and a third *o*, south following *g*."

" 10h 19'. I suspect a very small star, south following the " planet, at one-third of the distance of the 1st satellite; but " cannot verify it with 480. With 600, the same suspicion " continues."

" March 7. 9h 48'. The stars *a b c d e f g b k l* are in their " places."

" *n m o* are in their places."

" The planet has passed between the stars *e f*, pretty near " to *f*."

Reports.

March 21, 1794. Power 320. A small star was suspected south of the planet, or about 85° south following. It could not be verified with 480, nor with 600; and was even supposed to have been a deception; but the 2nd was found remaining in the place where the planet had left it.

Observations.

"March 26, 1794. 9^h 35′. With 480, I see the 1st satellite "much better than with 320. I suspected, with 320, a 3d sa- "tellite, directly north of the planet, a little farther off than the "1st, and this power almost verifies the suspicion." See figure 5. (Tab. III.)

"9^h 44′. With 600, I still suspect the same, but cannot sa- "tisfy myself of the reality."

"11^h 32′. I see the supposed 3d satellite perfectly well now. "It is much smaller than the 1st, and in a line with the planet "and the 1st; so that probably it is a fixed star; since it pre- "ceded the 1st, when I saw it before, I think more than the "quicker motion of the 1st satellite would account for. If it "be a fixed star, it makes almost a rectangular triangle with qr, "the shorter leg being 3d r; or it is almost in a line with q "and n."

"*N. B.* The lines in the description are truer than in the "figure, as the latter is only intended to point out the stars "in question."

"March 27. 8^h 37′. Power 320. The same small star, ob- "served last night at 11^h 32′, is gone from the place where I "saw it. From its light last night, compared to r, which

" to-night is very near the planet, and scarcely visible, I am " certain that it must be bright enough to be perceived imme- " diately, if it were in the place pointed out by my descrip- " tion."

" 10^h 20′. The planet is considerably removed from the " star *r*.

" 11^h 41′. I had many glimpses of small stars or supposed " satellites: one of them in a place agreeing with the 3d satel- " lite of last night, (supposing it to have moved with the planet;) " that is, a little farther off, and after the 1st. Another pre- " ceding the 1st, but nearer. Some others south, at a good dis- " tance; but not one of them could I see for any constancy. " They were only lucid glimpses."

Reports.

March 27, 1794. A supposed 4th satellite was delineated, but proved to be a star the 28th.

Observations.

" March 4, 1796. Configuration of the Georgian planet and " fixed stars for 10^h 3′." See fig. 6.

" March 5. 9^h 50′. I suspected a very small star between *c* " and *b*, which was not there last night. I had a pretty cer- " tain glimpse of it. It is in a line from the planet towards *f*: " power 320. With 600, I see the satellite better than before; " but cannot perceive the suspected small star."

" 10^h 17′. The air is remarkably clear at present, but I can- " not perceive the suspected star."

" March 9. 11^h 23′. As the probability of other satellites is, " that they revolve in the same plane with the 1st and 2d, I

"chiefly look for them in the direction of their orbits, which "is now nearly a straight line."

"April 5, 1796. There is no star in the line of the transverse, "that can be taken for a satellite: the evening is very beauti- "ful, and I examined that line with 300, at a distance; and "with 600, within the orbits of the two satellites."

"March 23, 1797. Three very small stars O P Q, are in the "path of the planet; they form an obtuse triangle."

"March 25. 11^h 4′. A very bright star S, at almost the dis- "tance of the field of view, is a little south of the path of the "planet. It has a small north preceding star T, which points "to two more V W, towards the north."

"Between the triangle of March 23d and the four last men- "tioned stars, is a very small star X."

"March 28. 10^h 52′. I see the stars S T V W X of March "25th."

"11^h 25′. From X towards the triangle O P Q of March 23d, "is an exceeding small star Y, about four times the distance "of the 2d satellite, and nearly in the line of the greatest elonga- "tion. I do not remember to have seen it the 25th."

"11^h 41′. The distance of Y from X is about $\frac{1}{4}$ of the dis- "tance of X from the triangle. It requires much attention to "see it; but I have a very complete view of it, by drawing "the planet just out of the field, and the star X almost on "the preceding side."

Arguments upon the Reports and Observations.

From the reports of the great number of supposed satellites, compared with the select observations which are given at length, it must be evident that the method of looking for difficult

objects, and of marking them down by lines and angles, with every other possible advantage for finding them again, has been completely understood and put in practice. So guarded against deceptions, we cannot but allow, that even a single glimpse of a very small star is a considerable argument in favour of its existence. What I call verifying a suspicion, which is generally done with a higher power than that which caused the suspicion, is obtaining a steadier view of the existence of the object in question; that is, to see it in such a manner as to be able to fix an eye upon it, and to compare it with other surrounding objects; and thus to be able to ascertain its relative situation with those other objects, in a satisfactory manner.

An interior Satellite.

The observation of Jan. 18, 1790, says, "a supposed 3d satel-"lite is about two diameters of the planet following." There is not the least doubt expressed about the existence of the satellite, or object in question, which therefore must be looked upon as ascertained. Now, the angle of the greatest elongation of the Georgian satellites, by my new tables, at the time of observation, was 81°33′ N.F. Therefore, the angle of the apogee was 8°27′ S.F.; and since, by observation, the satellite was "following," without any mention of degrees being made, we may admit it to have been not far from the parallel; suppose 11 or 12 degrees S.F. In this case, the satellite would be in the apogee about the time of the 2d observation, at $7^h\,57'$; which says, "I cannot perceive the satellite." But it will be shewn hereafter, when I come to treat of the vanishing of the satellites, that it would become invisible in this situation. Indeed, without the supposition of

the satellite's coming to the apogee, it might easily happen that the least change in the clearness of the air, during a time of $1^h\,5'$, which elapsed between the first and second observation, might render an object invisible, which, as the first observation says, was "excessively faint, and could only be seen by "glimpses."

From the observed distance, which is put at "2 diameters "of the planet," we may conclude what would be the distance of its greatest elongation. For, 2 diameters from the disk of the planet give $2\frac{1}{2}$ from the centre. Now, the distance of the apogee at this time, by my tables, was ,64, supposing that of the greatest elongation 1; therefore we have the radius of its orbit $\frac{2,5 \times 4'',12}{,64} = 16'',1$.

This calculation is not intended to determine precisely the distance of the satellite, but only to shew that its orbit is more contracted than that of the 1st, and that consequently it is an interior satellite.

If any doubt should be entertained about the validity of this observation, we have a second, and very striking one, of March 5, 1794; where an interior satellite was suspected south following the planet, at one-third of the distance of the 1st. March 4, when a description was made of the stars, as in figure 4, this satellite was not in the place where it was observed the 5th. And, by an examination of the same stars March 7, it appears, that even the smallest stars *n m o*, of the 5th, were seen in their former places, but not the satellite. The observation therefore must be looked upon as decisive with regard to its existence. If any doubt should arise, on account of the suspicion not being verified with 480, I must remark, that being used to such imperfect glimpses, it has generally

turned out, even when I have given up as improbable the existence of a supposed satellite seen in that manner, that it has afterwards nevertheless been discovered that a small star remained in the place where the satellite had been suspected to be situated. An instance of this may be seen in the report of the observations that were made March 21 and 22, 1794. Besides, in the present case, it is additionally mentioned, that the same object was examined with a power of 600, which continued the suspicion.

From the assigned place of this satellite, at $\frac{1}{3}$ of the distance of that of the first, it appears that this observation belongs to the interior satellite of Jan. 18, 1790, which has already been examined. The 1st satellite was this evening at its greatest elongation, one-third of which is about 11″. The apogee distance of a satellite whose greatest distance is 16″,1 would have been 6″,1 on the day of our observation; but, not being come to the apogee, by many degrees, it could not be so near the planet.

For the sake of greater precision, let us admit that the satellite was exactly south following; that is, 45 degrees from the parallel, and 45 from the meridian; then, by calculation, a satellite whose orbit is at 16″,1 from the planet, would, in the situation now admitted, have been 7″,1 from its centre, which might coarsely be rated at $\frac{1}{4}$ of the distance of the first. But the estimation of 11″ is probably more accurate than that in the 1st observation, where 2 diameters are given. And, by calculating from this quantity, we find that the greatest elongation distance of the satellite is 25″,5; now, putting $2\frac{1}{4}$ diameters in the first observation, instead of 2, the distance deduced from it will come out 19″,8; which is certainly an agreement

sufficiently near to admit both observations to belong to the same satellite.

March 27, 1794. We find a third observation, which will assist in supporting the two former ones. A glimpse of a satellite is mentioned, which was preceding the 1st, but nearer the planet. The position of the 1st satellite the same evening was, by measuring, found to be 62°,1 N.F. which is still a considerable way from its greatest elongation; but our new satellite preceded it, and was therefore more advanced in its orbit, or nearer its greatest distance; and yet the observation says, that it was not so far from the planet as the 1st; notwithstanding this latter was in a more contracted part of its orbit. It follows therefore that this was also an interior satellite. Now, since we may allow these three observations to belong to the same, we ought not to make a distinction; but admit, as sufficiently established, the existence of at least one interior satellite of our new planet.

An intermediate Satellite.

March 26, 1794. A satellite was suspected, directly north of the planet. At first it could not be verified, but was seen perfectly well afterwards. It was supposed that probably it might be a star, but this was left undecided. The observation of March 27th however removes all doubt upon the subject; as it fully affirms that the small star observed the 26th, at $11^h\,32'$, was gone from the place in which it was the day before. Such strong circumstances are mentioned in confirmation, that we cannot hesitate placing this among the list of existing satellites. It was not the interior satellite of Jan. 18, 1790; for both the

1st and 2d known satellites were in full view March 26th; see figure 5. and the observation places this new one in a line drawn from the planet continued through the 1st; with the remark, that it was a little farther from the planet than the 1st. The 2d was then near its greatest southern elongation, and we may see from the figure, as well as from the above description, that the orbit of this new satellite is situated between the orbits of the other two.

We have a second observation of the same satellite March 27, 1794; where, among the glimpses of additional satellites at $11^h\,41'$, is mentioned "one in a place probably agreeing with "the new satellite of March 26th," which, by its motion, must have been carried forward, so as to be where the observation of the 27th says it was, namely, "a little farther off and after the "1st;" that is, at a little greater distance from the planet than the 1st, and not so far advanced in its orbit as that satellite. This amounts not only to an additional proof, but even announces the recognition of the satellite, and its motion in the course of one day.

An exterior Satellite.

Feb. 9, 1790. A new satellite was seen, in a line with the planet and the 2d satellite. See figure 1. To convince us that this was not a fixed star, we have the observations of two other nights, the 11th and 12th of February, where the removal of it, from the place in which it was Feb. 9, is clearly demonstrated. As it was in a line continued from the planet through the second satellite, its orbit must evidently be of a greater dimension than that of the 2d; I shall therefore put it down as an exterior satellite.

Most likely this satellite also was seen among the supposed satellites south of the planet, March 27, 1794; where we find mention made of "some others south, at a good distance." In that case, this will make a second observation.

We have a third observation of the same new satellite March 5, 1796; when a very small star was seen, in a place where the evening before there had been none; as appears by the configuration of the 5th of March. See figure 6. At the time of the observation, the planet was come to the longitude of the place where the star was perceived to be; which agrees with the idea of its having been brought to that situation by the planet. It may be objected, that the star could not be verified with a power of 600; but here we have more than a bare suspicion of the satellite, for the observation says, "I had a pretty *certain* "glimpse of it;" and this appears also from the assigned place of the star at the intersection of two given lines. For, such a delineation could not have been made, without having perceived it with a considerable degree of steady vision. Its distance, to judge by the description, will agree sufficiently with the two foregoing observations of this new exterior satellite.

The most distant Satellite.

On Feb. 28, 1794, a star was perceived where on the 26th there was none. This star was larger than a very small star which was observed the 26th, not far from the place of the new supposed satellite; and a configuration having been made expressly, by way of ascertaining what stars might afterwards come into a situation where they could be mistaken for satellites, our new star or satellite would not have been omitted,

when a smaller one very near it was scrupulously recorded. The motion of the planet, in 3 hours and 3 minutes, is mentioned as very visible. The place of the star, which was a new visitor this evening, was very particularly delineated, at 6^h 50′. From its situation, it is evident the motion of the planet must have carried this star, if it was one of its satellites, towards the large star *f*, figure 3; in the light of which a dim satellite would be lost. This accordingly happened; for at 10^h 7′ and 10^h 21′ it was no longer visible. The direction of the planet's motion is plainly pointed out, by the place of the planet March 2d.

With respect to the orbit of this satellite, it appears, from its situation near the apogee, where it was seen, that its distance was to that of the second satellite, which was then near its greatest elongation, as 8 to 5. And, since the apogee distance, on the day of observation, was only ,37, we have its greatest elongation as $\frac{8}{,37}$ to 5; that is, as 21,6 to 5, or above 4 to 1. From which we may conclude, that its orbit must lie considerably without the before mentioned exterior satellite of Feb. 9, 1790.

We have a second observation of it March 27, 1794; which, though not very strong, yet adds confirmation to the former. For that evening, which was uncommonly fine, other satellites, south, at a good distance, were perceived. This must relate principally to our present satellite, which may certainly be said to be at a good distance from the planet, and which, by that time, was probably in the southern part of its orbit, and near its greatest elongation.

There is a third observation, March 28, 1797, which probably also belongs to this satellite. For the exceedingly small star Y,

which is mentioned as not having been seen the 25th, when the delineation of the stars was made, will agree very well with the two former observations; and, being near the greatest elongation, the distance of this satellite is well pointed out, and agrees remarkably well with the calculation of the first observation of it.

It remains now only to be mentioned, that in such delicate observations as these of the additional satellites, there may possibly arise some doubts with those who are very scrupulous; but, as I have been much in the habit of seeing very small and dim objects, I have not been detained from publishing these observations sooner, on account of the least uncertainty about the existence of these satellites, but merely because I was in hopes of being able soon to give a better account of them, with regard to their periodical revolutions. It did not appear satisfactory to me to announce a satellite, unless I could, at the same time, have pointed out more precisely the place where it might be found by other astronomers. But, as more time is now already elapsed than I had allowed myself for a completion of the theory of these satellites, I thought it better not to defer the communication any longer.

The arrangement of the four new and the two old satellites together will be thus:

First satellite, the interior one of Jan. 18, 1790.
Second satellite, the nearest old one of Jan. 11, 1787.
Third satellite, the intermediate one of March 26, 1794.
Fourth satellite, the farthest old one of Jan. 11, 1787.
Fifth satellite, the exterior one of Feb. 9, 1790.
Sixth satellite, the most distant one of Feb. 28, 1794.

*Observations and Reports tending to the Discovery of one or more Rings of the Georgian Planet, and the flattening of its polar Regions.**

"Nov. 13, 1782. 7-feet reflector, power 460. I perceive no "flattening of the polar regions."

"April 8, 1783. I surmise a polar flattening."

"Feb. 4, 1787. 20-feet reflector, power 300. Well defined; "no appearance of any ring; much daylight."

"March 4. I begin to entertain again a suspicion that the "planet is not round. When I see it most distinctly, it appears "to have double, opposite points. See figure 7. Perhaps a "double ring; that is, two rings, at rectangles to each other."

March 5. The Georgian Sidus not being round, the telescope was turned to Jupiter. I viewed that planet with 157, 300, and 480, which shewed it perfectly well defined. Returning to the Georgian planet, it was again seen affected with projecting points. Two opposite ones, that were large and blunt, from preceding to following; and two others, that were small and less blunt, from north to south. See figure 7.

March 7. Position of the great ring R, from 70° S.P. to 70° N.F. Small ring *r*, from 20° N.P. to 20° N.F. 600 shewed R and *r*. 800 R and *r*. 1200 R and *r*.

"March 8. R and *r* are probably deceptions."

"Nov. 9. The suspicion of a ring returns often when I ad- "just the focus by one of the satellites, but yet I think it has "no foundation."

Feb. 22, 1789. A ring was suspected.

* The observations are distinguished from the reports by marks of quotation, (" ").

"March 16. 7^h 37'. I have turned my speculum 90° round. "A certain appearance, owing to a defect which it has con- "tracted by exposure to the air since it was made, is gone "with it; (see fig. 9 and 10;) but the suspected ring remains in the place where I saw it last.

"7^h 50'. Power 471 shews the same appearance rather "stronger. Power 589 still shews the same."

"*Memorandum.* The ring is short, not like that of Saturn. "It seems to be as in figure 8; and this may account for the "great difficulty of verifying it. It is remarkable that the two "*ansæ* seem of a colour a little inclined to red. The blur oc- "casioned by the fault of the speculum is, to-night, as repre- "sented in figure 9. The other evening it was as in figure 10; "and the ring is likewise as it was the same evening."

"March 20. 7^h 59'. When the satellites are best in focus, "the suspicion of a ring is the strongest."

"Dec. 15. The planet is not round, and I have not much "doubt but that it has a ring."

"Feb. 26, 1792. 6^h 34'. My telescope is extremely distinct; "and, when I adjust it upon a very minute double star, which "is not far from the planet, I see a very faint ray, like a ring "crossing the planet, over the centre. This appearance is of "an equal length on both sides, so that I strongly suspect it "to be a ring. There is, however, a possibility of its being an "imperfection in the speculum, owing to some slight scratch: "I shall take its position, and afterwards turn the speculum on "its axis."

"8^h 39'. Position of the supposed ring 55°,6 from N.P. to S.F."

"9^h 56'. I have turned the speculum one quadrant round; "but the appearance of the very faint ray continues where it

" was before, so that the defect is not in the speculum, nor is " it in the eye-glass. But still it is now also pretty evident " that it arises from some external cause; for it is now in the " same situation, with regard to the tube, in which it was $3\frac{1}{2}$ " hours ago: whereas, the parallel is differently situated, and " the ring, of course, ought to be so too."

" March 5, 1792. I viewed the Georgian planet with a newly " polished speculum, of an excellent figure. It shewed the pla- " net very well defined, and without any suspicion of a ring. " I viewed it successively with 240, 300, 480, 600, 800, 1200, " and 2400; all which powers my speculum bore with great dis- " tinctness. I am pretty well convinced that the disk is flat- " tened." The moon was pretty near the planet.

" Dec. 4, 1793. 7-feet reflector, power 287. The Georgian " planet is not so well defined as, from the extraordinary dis- " tinctness of my present 7-feet telescope, it ought to be. There " is a suspicion of some apparatus about the planet."

" Feb. 26, 1794. 20-feet reflector, power 480. The planet " seems to be a little lengthened out, in the direction of the " longer axis of the satellites' orbits."

" April 21, 1795. 10-feet reflector, power 400. The telescope " adjusted to a neighbouring star, so as to make it perfectly " round. The disk of the planet seems to be a little elliptical. " With 600, also adjusted upon the neighbouring star, the disk " still seems elliptical."

Remarks upon the foregoing Observations.

With regard to the phænomena which gave rise to the suspicion of one or more rings, it must be noticed, that few specula or object-glasses are so very perfect as not to be affected

with some rays or inequalities, when high powers are used, and the object to be viewed is very minute. It seems, however, from the observations of March 16, 1789, and Feb. 26, 1792, that the cause of deception, in this case, must be looked for elsewhere. It has often happened, that the situation of the eye-glass, being on one side of the tube, which brings the observer close to the mouth of it, has occasioned a visible defect in the view of a very minute object, when proper care has not been taken to keep out of the way; especially when the wind is in such a quarter as to come from the observer across the telescope. The direction of a current of air alone may also affect vision. Without, however, entering further into the discussion of a subject that must be attended with uncertainty, I will only add, that the observation of the 26th seems to be very decisive against the existence of a ring. When the surmises arose at first, I thought it proper to suppose, that a ring might be in such a situation as to render it almost invisible; and that, consequently, observations should not be given up, till a sufficient time had elapsed to obtain a better view of such a supposed ring, by a removal of the planet from its node. This has now sufficiently been obtained in the course of ten years; for, let the node of the ring have been in any situation whatsoever, provided it kept to the same, we must by this time have had a pretty good view of the ring itself. Placing therefore great confidence on the observation of March 5, 1792, supported by my late views of the planet, I venture to affirm, that it has no ring in the least resembling that, or rather those, of Saturn.

The flattening of the poles of the planet seems to be sufficiently ascertained by many observations. The 7-feet, the

10-feet, and the 20-feet instruments, equally confirm it; and the direction pointed out Feb. 26, 1794, seems to be conformable to the analogies that may be drawn from the situation of the equator of Saturn, and of Jupiter.

This being admitted, we may without hesitation conclude, that the Georgian planet also has a rotation upon its axis, of a considerable degree of velocity.

Reports and Observations relating to the Light and Size of the Georgian Satellites, and to their vanishing at certain Distances from the Planet.

Jan. 14, 1787. A star was put down, as a supposed very faint satellite; but, on the 17th, the planet being removed, it appeared nearly as bright as two considerable stars that had also been noted.

" Jan. 17. The 1st satellite is the smallest in appearance."

" Jan. 24. The 2d satellite is brighter than the first."

" Feb. 9, 1787. The 1st satellite is larger than the second."

Feb. 10. The planet was supposed to go to a triangle of pretty bright stars. The 11th it was between them, and the stars of the triangle were so dim, that, had they not been seen before, they might have been supposed to be satellites.

" Sept. 19, 1787. 4^h 24'. I can still see the satellites, though " daylight is already very strong: they are fainter than the " faintest of Saturn's satellites." *

" Feb. 22, 1791. I cannot perceive the 1st satellite, probably " owing to its nearness to the planet."

" March 2, 1791. The 1st satellite is hardly to be seen; I

* Five satellites of Saturn were only known at that time.

" have however had several perfect glimpses of it. It seems to " be about the most contracted part of its orbit."

March 6. The supposed 3d and 4th satellites of March 5th were imagined to have been gone from their former places; but were seen the 7th, with this memorandum. " I mistook them last night for other stars, they being so large that I did not know them again."

" March 9. The 2d satellite is nearer the planet than the " first, and on that account appears smaller."

" Dec. 9, 1791. I do not perceive the 1st satellite."

" Feb. 13, 1792. $6^h\ 16'$. The 3d supposed satellite of last " night is a considerable star; not much less than *b*."

When the supposed third was pointed out the night before, it is said to be smaller than the 1st and 2d satellites. By the figure, it did not exceed the distance of the 2d; and *b* is called a pretty large star.

Feb. 20, 1792. The 2d satellite, being at a great distance, was mistaken for a pretty large star, till about four hours after, when its motion along with the planet was perceived.

" Feb. 21, 1792. $7^h\ 36'$. I cannot see the 2d satellite. By " calculation, it should be about $8',6$ S.F. and I suspect it to " be there, but cannot get the least assurance."

" March 15, 1792. I cannot see the 1st satellite with 300; nor with 480; nor with 600."

" March 19. $8^h\ 35'$. I cannot see the 2d satellite with 300. " With 480 I see it very well. I see it also with 800; and very " well with 1200. With 2400 and 4800 the satellite cannot be " seen; but there seems to be a whitish haziness coming on."

March 4, 1794. The 1st satellite could not be seen.

March 7. The 1st was invisible.

March 17. Both 1st and 2d were invisible.

March 21. The 1st was invisible, though looked for with all the powers of the instrument.

March 22. The 2d was hardly visible.

March 23. The 2d was not to be seen.

March 26. The 1st was but just visible.

March 5, 1796. The 2d was invisible.

April 4, 1796. The 1st was invisible.

"March 17, 1797. Power 600. Neither of the satellites are "visible to-night; with 300 I cannot see them. The night is "very beautiful, and I have a field bar to hide the planet; but, "notwithstanding this, I cannot see either of the satellites."

March 21. The 1st satellite was invisible.

March 23. The 2d was invisible. The 1st could not be seen immediately, but, having been informed where exactly to look for it, according to my calculation of its place, it was perceived; and with 600 seen very well.

March 25. Both satellites were invisible.

Remarks on the foregoing Observations.

From the observations of Jan. 14, Feb. 10, March 6, 1787, and Feb. 13, 1792, it appears, that all very small stars, when they come near the planet, lose much of their lustre. Indeed, every observation that has been recorded before, of supposed satellites that have been proved to be stars afterwards, has fully confirmed this circumstance; for they were always found to be considerable stars, and their being mistaken for satellites was owing to their loss of light when near the planet. This would hardly deserve notice, as it is well known that a superior light

will obstruct an inferior one; but some circumstances which attend the operation of the affections of light upon the eye, when objects are very faint, are so remarkable, that they must not be passed over in silence.

After having been used to follow up the satellites of Saturn and Jupiter, to the very margin of their planets, so as even to measure the apparent diameter of one of Jupiter's satellites by its entrance on the disk,* I was in hopes that a similar opportunity would soon have offered with the Georgian satellites: not indeed to measure the satellites, but to measure the planet itself, by means of the passage of the satellite over its disk. I expected also to have settled the epochs of the satellites, from their conjunctions and oppositions, with more accuracy than I have yet been able to do, from their various positions in other parts of their orbits. A disappointment of obtaining these capital advantages deserves to have its cause investigated; but, first of all, let us cast a look upon the observations.

The satellites, we may remark, become regularly invisible, when, after their elongation, they arrive to certain distances from the planet. In order to find what these distances are, we will take the first observation of this kind, as an example.

Feb. 22, 1791, the first satellite could not be seen. Now, by my lately constructed tables, its longitude from the apogee, at the time of observation, was 204,5 degrees; that is, 24,5 degrees from the most contracted part of its orbit, on the side that is turned to us, which, as its opposite is called the apogee, I shall call the perigee. By my tables also for the same day, we have the distance of the apogee from the planet, which is ,60; supposing the greatest elongation distance to be 1. This

* See Phil. Trans. for 1797, Part II. page 335.

being given, we may find an easy method of ascertaining the distance of the satellite, when it is near the apogee or perigee: for it will be sufficiently true for our purpose to use the following analogy. Cosine of the distance of the satellite from the apogee or perigee is to the apogee distance from the planet, as the greatest elongation is to the distance of the satellite from the planet. When the ellipsis is very open, this theorem will only hold good in moderate distances from the apogee or perigee; but, when it is a good deal flattened, it will not be considerably out in more distant situations: and it will also be sufficiently accurate to take the natural cosine from the tables to two places of decimals only. When this is applied to our present instance, we have ,91 for the natural cosine of 24°,5 degrees; and the distance of the satellite from the planet will come out $\frac{,6 \times 33'}{,91} = 21'',8$.

By this method, it appears that the satellite, when it could not be seen, was nearly 22" from the planet.

We must not however conclude, that this is the given distance at which it will always vanish. For instance, the same satellite, though hardly to be seen, was however not quite invisible March 2, 1791. Its distance from the planet, computed as before, was then only $\frac{,6 \times 33'}{1} = 19'',8$.

The clearness of the atmosphere, and other favourable circumstances, must certainly have great influence in observations of very faint objects; therefore, a computation of all the observations where the satellites were not seen, as well as a few others where they were seen, when pretty near the apogee or perigee, will be the surest way of settling the fact. The result of these computations is thus.

First satellite invisible.				Second satellite invisible.			
1791.	Feb. 22	at	21″,8	1792.	Feb. 21	at	23″,8
	Dec. 19	at	16,9	1794.	March 17	at	20,7
1792.	March 15	at	18,4		March 23	at	17,9
1794.	March 4	at	18,5	1796.	March 5	at	9,8
	March 7	at	12,5		March 17	at	6,8
	March 17	at	17,0		March 23	at	6,2
	March 21	at	15,5		March 25	at	8,7
	April 4	at	8,5				
1797.	March 17	at	4,8				
	March 21	at	4,6				
	March 25	at	4,8				
First satellite visible.				Second satellite visible.			
1791.	March 2	at	19″,8	1794.	March 22	at	17″,5
1794.	Feb. 26	at	14,1				

Thus, having the observations and calculated distances under our inspection, we find that both the satellites became always invisible when they were near the planet: that the 1st was generally lost when it came within 18″ of the planet, and the 2d at the distance of about 20″. In very uncommon and beautiful nights, the 1st has once been seen at 13″,8, and the 2d at 17″,3; but at no time have they been visible when nearer the planet.

I shall now endeavour to investigate the cause which can

render small stars and satellites invisible at so great a distance as 18 or 20″.

A dense atmosphere of the planet would account for the defalcation of light sufficiently, were it not proved that the satellites are equally lost, whether they are in the nearest half of their orbits, or in that which is farthest from us. But, as a satellite cannot be eclipsed by an atmosphere that is behind it, a surmise of this kind cannot be entertained. Let us then turn our view to light itself, and see whether certain affections between bright and very bright objects, contrasted with others that take place between faint and very faint ones, will not explain the phænomena of vanishing satellites.

The light of Jupiter or Saturn, for instance, on account of its brilliancy, is diffused, almost equally, over a space of several minutes all around these planets. Their satellites also, having a great share of brightness, and moving in a sphere that is strongly illuminated, cannot be much affected by their various distances from the planets. The case then is, that they have much light to lose, and comparatively lose but little.

The Georgian planet, on the contrary, is very faint; and the influence of its feeble light cannot extend far, with any degree of equality. This enables us to see the faintest objects, even when they are only a minute or two removed from it. The satellites of this planet are very nearly the dimmest objects that can be seen in the heavens; so that they cannot bear any considerable diminution of their light, by a contrast with a more luminous object, without becoming invisible. If then the sphere of illumination of our new planet be limited to 18 or 20″, we may fully account for the loss of the satellites when they come

within its reach; for they have very little light to lose, and lose it pretty suddenly.

This contrast, therefore, between the condition of the Georgian satellites and those of the brighter planets, seems to be sufficient to account for the phænomenon of their becoming invisible.

We may avail ourselves of the observations that relate to the distances at which the satellites vanish, to determine their relative brightness. The 2d satellite appears generally brighter than the 1st; but, as the former is usually lost farther from the planet than the latter, we may admit the 1st satellite to be rather brighter than the 2d. This seems to be confirmed by the observation of March 9, 1791; where the 2d appeared to be smaller than the 1st, though the latter was only 25″ from the planet, while the other was 30″,8.

The first of the new satellites will hardly ever be seen otherwise than about its greatest elongations, but cannot be much inferior in brightness to the other two; and, if any more interior satellites should exist, we shall probably not obtain a sight of them; for the same reason that the inhabitants of the Georgian planet perhaps never can discover the existence of our earth, Venus, and Mercury.

The 2d new or intermediate satellite is considerably smaller than the 1st and 2d old satellites. The two exterior, or 5th and 6th satellites, are the smallest of all, and must chiefly be looked for in their greatest elongations.

Periodical Revolutions of the new Satellites.

It may be some satisfaction to know what time the four

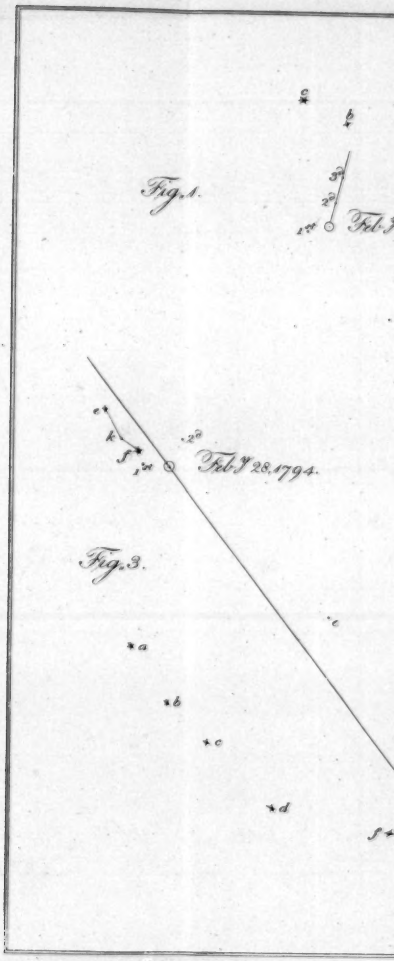


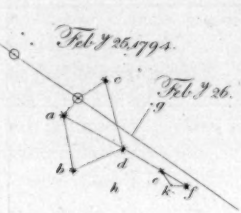
Fig. 1.
Feb. 28. 1794.
Fig. 3.
a
b
c
d

d

a

Feb.ʸ 9, 1790.

Fig. 2.

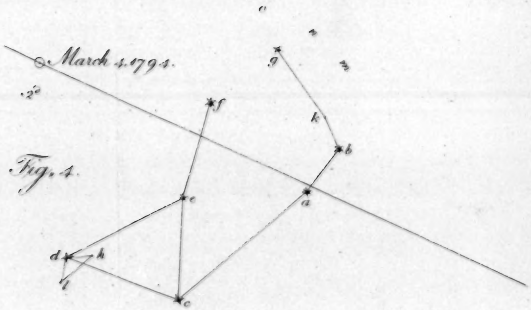


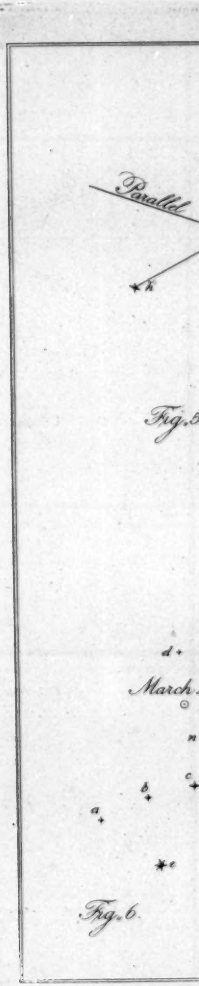
g

March 2.

f 4

Fig. 4.




Parallel
Fig. 5
d
March
n
c
b
a
e
Fig. 6.

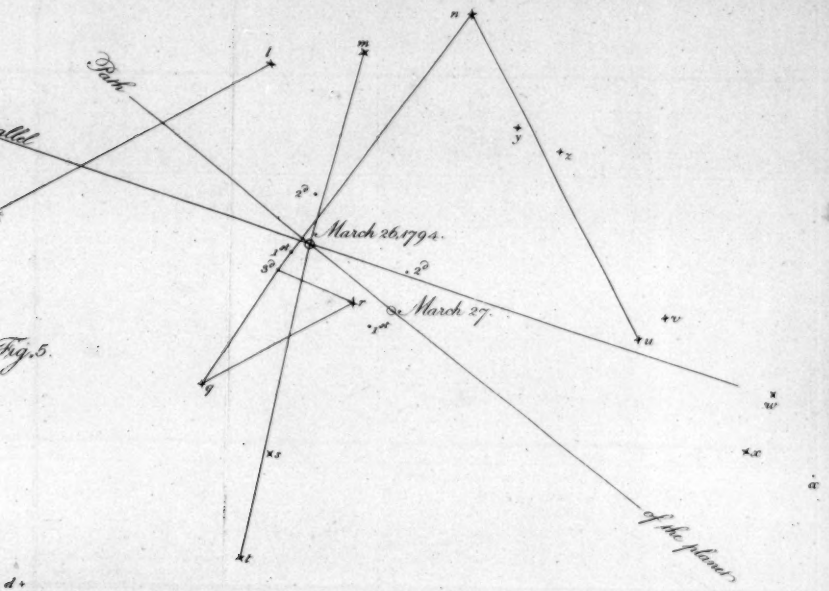


Fig. 5.

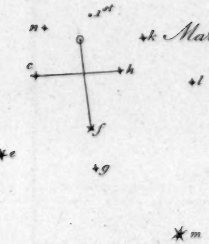


Fig. 7.

Fig. 8.

Fig. 9.

Fig. 10.

additional satellites probably employ in revolving round their planet. Now, as this can only be ascertained with accuracy by many observations, we must of course remain in suspense, till a series of them can be properly instituted. But, in the mean time, we may admit the distance of the interior satellite to be 25″,5, as our calculation of the estimation of March 5, 1794, gives it; and from this we compute that its periodical revolution will be 5 days, 21 hours, 25 minutes.

If we place the intermediate satellite at an equal distance between the two old ones, or at 38″,57, its period will be 10 days, 23 hours, 4 minutes.

By the figure of Feb. 9, 1790, it seems that the nearest exterior satellite is about double the distance of the farthest old one; hence, its periodical time is found to be 38 days, 1 hour, 49 minutes.

The most distant satellite, according to the calculation of the observation of Feb. 28, 1794, is full four times as far from the planet as the old 2d satellite; it will therefore take at least 107 days, 16 hours, 40 minutes, to complete one revolution.

It will hardly be necessary to add, that the accuracy of these periods depends entirely upon the truth of the assumed distances; some considerable difference, therefore, may be expected, when observations shall furnish us with proper *data* for more accurate determinations.

Slough, near Windsor,
September 1, 1797.

IV. *An Inquiry concerning the Source of the Heat which is excited by Friction.* By Benjamin *Count of* Rumford, *F. R. S. M. R. I. A.*

Read January 25, 1798.

It frequently happens, that in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of nature; and very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life has oftener led, as it were by accident, or in the playful excursions of the imagination, put into action by contemplating the most common appearances, to useful doubts, and sensible schemes for investigation and improvement, than all the more intense meditations of philosophers, in the hours expressly set apart for study.

It was by accident that I was led to make the experiments of which I am about to give an account; and, though they are not perhaps of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in superintending the boring of cannon, in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat which a brass gun acquires, in a short time, in being bored; and with the still more intense heat (much greater than that of boiling water, as I found by experiment,) of the metallic chips separated from it by the borer.

The more I meditated on these phænomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of heat; and to enable us to form some reasonable conjectures respecting the existence, or non-existence, of an *igneous fluid*: a subject on which the opinions of philosophers have, in all ages, been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the heat actually produced in the mechanical operation above mentioned?

Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent heat, and of caloric, the *capacity for heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the heat produced.

But no such change had taken place; for I found, upon

taking equal quantities, by weight, of these chips, and of thin slips of the same block of metal separated by means of a fine saw, and putting them, at the same temperature, (that of boiling water,) into equal quantities of cold water, (that is to say, at the temperature of $59°\frac{1}{2}$ F.) the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion, in which the slips of metal were put.

This experiment being repeated several times, the results were always so nearly the same, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its capacity for heat*, by being reduced to chips by the borer.*

From hence it is evident, that the heat produced could not

* As these experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows:

To 4590 grains of water, at the temperature of $59°\frac{1}{2}$ F. (an allowance as compensation, reckoned in water, for the capacity for heat of the containing cylindrical tin vessel, being included,) were added $1016\frac{1}{2}$ grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well stirred about, by means of a small rod of light wood, the heat of the mixture was found to be = 63°.

From this experiment, the *specific heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be = 0.1160, that of water being = 1.0000.

An experiment was afterwards made with the metallic chips, as follows:

To the same quantity of water as was used in the experiment above mentioned, at the same temperature, (*viz.* $59°\frac{1}{2}$,) and in the same cylindrical tin vessel, were now put $1016\frac{1}{2}$ grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature (210°). The heat of the mixture, at the end of 1 minute, was just 63°, as before; consequently the specific heat of these metallic chips was = 0.1100. Each of the above experiments was repeated 3 times, and always with nearly the same results.

possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment.

Taking a cannon, (a brass six-pounder,) cast solid, and rough as it came from the foundry, (see fig. 1. Tab. IV.) and fixing it (horizontally) in the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see fig. 2.) I caused its extremity to be cut off; and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{1}{2}$ inches in diameter, and $9\frac{1}{10}$ inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon,) by a small cylindrical neck, only $2\frac{1}{2}$ inches in diameter, and $3\frac{1}{10}$ inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in fig. 2; as also in fig. 3. where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating heat *by friction*, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumu-

lated in the cylinder might from time to time be measured, a small round hole, (see *d*, *e*, fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were 385$\frac{1}{2}$ cubic inches, English measure; and it weighed 113.13lb. avoirdupois: as I found, on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected.*

Experiment No. 1.

This experiment was made in order to ascertain how much heat was actually generated by friction, when, a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder, that the pressure

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorner kopf*, (the head of the cannon to be thrown away,) and which is represented in fig. 1.

This additional projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

against it was equal to the weight of about 10000lb. avoirdupois, the cylinder was turned round on its axis, (by the force of horses,) at the rate of about 32 times in a minute.

This machinery, as it was put together for the experiment, is represented by fig. 2. W is a strong horizontal iron bar, connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{4}{5}$ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (m) which kept it in its place. The area of the surface by which its end was in contact with the bottom of the bore of the cylinder was nearly $2\frac{1}{3}$ inches. This borer, which is distinguished by the letter n, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{13}{100}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the

hole made to receive it, in the side of the cylinder, when the mercury rose almost instantly to 130°.

Though the heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended from the axis of the cylinder to near its surface, the heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it, during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it.

Thus, at the end of	The heat, as shown by the thermometer, was
4 minutes - - -	126°
after 5 minutes, always reckoning from the first observation, - -	125°
at the end of 7 minutes - - -	123°
12 —— - - -	120°
14 —— - - -	119°
16 —— - - -	118°
20 —— - - -	116°
24 —— - - -	115°
28 —— - - -	114°
31 —— - - -	113°
34 —— - - -	112°
37½ —— - - -	111°
and when 41 minutes had elapsed - -	110°

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 11 9lb. of gun-metal at least 70 degrees of FAHRENHEIT's thermometer, and which, of course, would have been capable of melting 6½lb. of ice, or of causing near 5lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of *a change* of its capacity for heat?

As the weight of this dust (837 grains Troy) amounted to no more than $\frac{1}{5\,4\,1\,8}$th part of that of the cylinder, it must have lost no less than 948 degrees of heat, to have been able to have raised the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of heat, to have produced the effects which were actually found to have been produced in the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, *is not sensibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces, by means of a borer that is less sharp.

If the heat, or any considerable part of it, were produced in consequence of a change in the capacity for heat of a part of the metal of the cylinder, as such change could only be *superficial*, the cylinder would by degrees be *exhausted*; or the quantities of heat produced, in any given short space of time, would be found to diminish gradually, in successive experiments. To find out if this really happened or not, I repeated the last-mentioned experiment several times, with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of heat actually given off.

Finding so much reason to conclude, that the heat generated in these experiments, or *excited*, as I would rather choose to express it, was not furnished *at the expence of the latent heat* or *combined caloric* of the metal, I pushed my inquiries a step farther, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

Experiment No. 2.

As the bore of the cylinder was cylindrical, and as the iron bar, (*m*,) to the end of which the blunt steel borer was fixed, was square, the air had free access to the inside of the bore, and even to the bottom of it, where the friction took place by which the heat was excited.

As neither the metallic chips produced in the ordinary course of the operation of boring brass cannon, nor the finer scaly particles produced in the last mentioned experiments by the friction of the blunt borer, showed any signs of calcination, I did not see how the air could possibly have been the cause

of the heat that was produced; but, in an investigation of this kind, I thought that no pains should be spared to clear away the rubbish, and leave the subject as naked and open to inspection as possible.

In order, by one decisive experiment, to determine whether the air of the atmosphere had any part, or not, in the generation of the heat, I contrived to repeat the experiment, under circumstances in which *it was evidently impossible for it to produce any effect whatever*. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the access of the external air, to the inside of the bore of the cylinder, was effectually prevented. (In fig. 3. this piston (*p*) is seen in its place; it is likewise shown in fig. 7 and 8.)

I did not find, however, by this experiment, that the exclusion of the air diminished, in the smallest degree, the quantity of heat excited by the friction.

There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was however desirous to remove. The piston which closed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction, when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came in contact with the piston, is it not possible that

this air may have had some share in the generation of the heat produced?

Experiment No. 3.

A quadrangular oblong deal box, (see fig. 4.) water-tight, $11\frac{1}{2}$ English inches long, $9\frac{4}{10}$ inches wide, and $9\frac{6}{10}$ inches deep, (measured in the clear,) being provided, with holes or slits in the middle of each of its ends, just large enough to receive, the one, the square iron rod to the end of which the blunt steel borer was fastened, the other, the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above, by a wooden cover or lid moving on hinges,) was put into its place; that is to say, when, by means of the two vertical openings or slits in its two ends, (the upper parts of which openings were occasionally closed, by means of narrow pieces of wood sliding in vertical grooves,) the box (*g*, *b*, *i*, *k*, fig. 3.) was fixed to the machinery, in such a manner that its bottom (*i*, *k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the description, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either side, (as it is represented in fig. 3.;) and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and surrounded on every side, by that fluid. And farther, as the box was held fast by the strong square iron rod, (*m*,) which passed, in a *square hole*, in the centre of one of its ends, (*a*, fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put

in motion, without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the experiment I had projected, in the following manner.

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being closed at the same time, by means of the circular piston, through the centre of which the iron bar passed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60°.) and the machine was put in motion.

The result of this beautiful experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery used in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water, and touching the outside of the cylinder, that heat was generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77lb. avoirdupois, or $2\frac{1}{4}$ wine gallons,) that its temperature

had been raised no less than 47 degrees; being now 107° of FAHRENHEIT's scale.

When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was 142°.

At the end of 2 hours, reckoning from the beginning of the experiment, the temperature of the water was found to be raised to 178°.

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to describe the surprise and astonishment expressed in the countenances of the by-standers, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could justly be considered as surprising in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure, which, were I ambitious of the reputation of a *grave philosopher*, I ought most certainly rather to hide than to discover.

The quantity of heat excited and accumulated in this experiment was very considerable; for, not only the water in the box, but also the box itself, (which weighed 15¼lb.) and the hollow metallic cylinder, and that part of the iron bar which, being situated within the cavity of the box, was immersed in the water, were heated 150 degrees of FAHRENHEIT's scale; *viz.* from 60° (which was the temperature of the water, and of the machinery, at the beginning of the experiment,) to 210°, the heat of boiling water at Munich.

The total quantity of heat generated may be estimated with some considerable degree of precision, as follows:

Quantity of ice-cold water which, with the given quantity of heat, might have been heated 180 degrees, or made to boil. In avoirdupois weight.

Of the heat excited there appears to have been actually accumulated,

In the water contained in the wooden box, 18¼ lb. avoirdupois, heated 150 degrees, namely, from 60° to 210° F. - - - lb. 15.2

In 113.13 lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated 12½ lb. of water the same number of degrees - - - 10.87

In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for heat to 1.21 lb. of water 1.01

N. B. No estimate is here made of the heat accumulated in the wooden box, nor of that dispersed during the experiment.

Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees, or made to boil - - - 26.58

From the knowledge of the *quantity* of heat actually produced in the foregoing experiment, and of the *time* in which it was generated, we are enabled to ascertain *the velocity of its production*, and to determine how large a fire must have been, or how much fuel must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of heat in the same time.

In one of Dr. CRAWFORD'S experiments, (see his Treatise on Heat, p. 321,) 37 lb. 7 oz. Troy, = 181920 grains, of water,

were heated $2\frac{1}{10}$ degrees of FAHRENHEIT's thermometer, with the heat generated in the combustion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{13}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains heated 180 degrees, with the heat generated in the combustion of 1 grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the heat generated by friction in the beforementioned experiment, was found to be 26.58lb. avoirdupois, = 188060 grains; and, as 81.631 grains of ice-cold water require the heat generated in the combustion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combustion of no less than 2303.8 grains ($= 4\frac{3}{10}$oz. Troy) of wax, to heat it 180 degrees.

As the experiment (No. 3.) in which the given quantity of heat was generated by friction, lasted 2 hours and 30 minutes, = 150 minutes, it is necessary, for the purpose of ascertaining how many wax candles of any given size must burn together, in order that in the combustion of them the given quantity of heat may be generated in the given time, and consequently *with the same celerity* as that with which the heat was generated by friction in the experiment, that the size of the candles should be determined, and the quantity of wax consumed in a given time by each candle, in burning equably, should be known.

Now I found by an experiment, made on purpose to finish these computations, that when a good wax candle, of a moderate size, $\frac{1}{4}$ of an inch in diameter, burns with a clear flame, just 49 grains of wax are consumed in 30 minutes. Hence it appears, that 245 grains of wax would be consumed by such a candle in 150 minutes; and that, to burn the quantity of

wax (= 2303.8 grains) necessary to produce the quantity of heat actually obtained by friction in the experiment in question, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be sufficient; for, 9 multiplied into 245 (the number of grains consumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax necessary to be burnt, in order to produce the given quantity of heat, was found to be 2303.8 grains.

From the result of these computations it appears, that the quantity of heat produced equably, or in a continual stream, (if I may use that expression,) by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, in the experiment under consideration, was *greater* than that produced equably in the combustion of *nine wax candles*, each $\frac{7}{8}$ of an inch in diameter, all burning together, or at the same time, with clear bright flames.

As the machinery used in this experiment could easily be carried round by the force of one horse, (though, to render the work lighter, two horses were actually employed in doing it,) these computations show further how large a quantity of heat might be produced, by proper mechanical contrivance, merely by the strength of a horse, without either fire, light, combustion, or chemical decomposition; and, in a case of necessity, the heat thus produced might be used in cooking victuals.

But no circumstances can be imagined, in which this method of procuring heat would not be disadvantageous; for, more heat might be obtained by using the fodder necessary for the support of a horse, as fuel.

As soon as the last mentioned experiment (No. 3.) was finished, the water in the wooden box was let off, and the box

removed; and the borer being taken out of the cylinder, the scaly metallic powder, which had been produced by the friction of the borer against the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{3}{5}$ oz. Troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 824 grains for the quantity produced *in half an hour*.

In the first experiment, which lasted only *half an hour*, the quantity produced was 837 grains.

In the experiment No. 1, the quantity of heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180 degrees, or cause it to boil.

According to the result of the experiment No. 3, the heat generated in *half an hour*, would have caused 5.31lb. of ice-cold water to boil. But, in this last-mentioned experiment, the heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two experiments.

It remains for me to give an account of one experiment more, which was made with this apparatus. I found by the experiment No. 1. how much heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the experiment No. 2, I found that the quantity of heat generated was not sensibly diminished when the free access of the air was prevented; and, by the result of No. 3, it appeared that the generation of the heat was not prevented, or retarded, by keeping the apparatus immersed in water. But as, in this last-mentioned experiment, the water, though it surrounded the hollow metallic cylinder on every

side, externally, was not suffered to enter the cavity of its bore, (being prevented by the piston,) and consequently did not come into contact with the metallic surfaces where the heat was generated; to see what effects would be produced by giving the water free access to these surfaces, I now made the

Experiment No. 4.

The piston which closed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the result of this experiment that renders it necessary for me to be very particular in my account of it. Heat was generated, as in the former experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the last. The only circumstance that surprised me was, to find how little difference was occasioned in the noise made by the borer in rubbing against the bottom of the bore of the cylinder, by filling the bore with water. This noise, which was very grating to the ear, and sometimes almost insupportable, was, as nearly as I could judge of it, quite as loud, and as disagreeable, when the surfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely,

What is heat?—Is there any such thing as an *igneous fluid?*—Is there any thing that can with propriety be called *caloric?*

We have seen that a very considerable quantity of heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux, *in all directions*, without interruption or intermission, and without any signs of diminution, or exhaustion.

From whence came the heat which was continually given off in this manner, in the foregoing experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for, in three of the experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident; *first*, because this water was continually *receiving heat* from the machinery, and could not, at the same time, be *giving to*, and *receiving heat from*, the same body; and *secondly*, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place, (which indeed could not reasonably have been expected,) one of its component elastic fluids (most probably inflammable air) must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but, though I frequently examined the water, to see if any air bubbles rose up through it, and had even made preparations for catching

them, in order to examine them, if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before mentioned; for heat was continually going off, or *out of the machinery*, by both these passages, during the whole time the experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction, in these experiments, appeared evidently to be *inexhaustible*.

It is hardly necessary to add, that any thing which any *insulated* body, or system of bodies, can continue to furnish *without limitation*, cannot possibly be a *material substance*: and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited, and communicated, in the manner the heat was excited and communicated in these experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been supposed to constitute heat, is excited, continued, and propagated, and I shall not presume to trouble the Society with mere conjectures; particularly on a subject which, during so many thousand years, the most

enlightened philosophers have endeavoured, but in vain, to comprehend.

But, although the mechanism of heat should, in fact, be one of those mysteries of nature which are beyond the reach of human intelligence, this ought by no means to discourage us, or even lessen our ardour, in our attempts to investigate the laws of its operations. How far can we advance in any of the paths which science has opened to us, before we find ourselves enveloped in those thick mists which, on every side, bound the horizon of the human intellect? But how ample, and how interesting, is the field that is given us to explore!

Nobody, surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal NEWTON enabled to make, merely by the investigation of the laws of its action!

The effects produced in the world by the agency of heat, are probably *just as extensive*, and quite as important, as those which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all cases, determined by laws equally immutable.

Before I finish this paper, I would beg leave to observe, that although, in treating the subject I have endeavoured to investigate, I have made no mention of the names of those who have gone over the same ground before me, nor of the success of their labours; this omission has not been owing to any want of respect for my predecessors, but was merely to avoid prolixity, and to be more at liberty to pursue, without interruption, the natural train of my own ideas.

DESCRIPTION OF THE FIGURES (Tab. IV.)

Fig. 1. shows the cannon used in the foregoing experiments, in the state it was in when it came from the foundry.

Fig. 2. shows the machinery used in the experiments No. 1, and No. 2. The cannon is seen fixed in the machine used for boring cannon. W is a strong iron bar, (which, to save room in the drawing, is represented as broken off,) which bar, being united with machinery (not expressed in the figure) that is carried round by horses, causes the cannon to turn round its axis.

m is a strong iron bar, to the end of which the blunt borer is fixed; which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating heat by friction.

Fig. 3. shows, on an enlarged scale, the same hollow cylinder that is represented on a smaller scale in the foregoing figure. It is here seen connected with the wooden box (*g*, *b*, *i*, *k*,) used in the experiments No. 3, and No. 4, when this hollow cylinder was immersed in water.

p, which is marked by dotted lines, is the piston which closed the end of the bore of the cylinder.

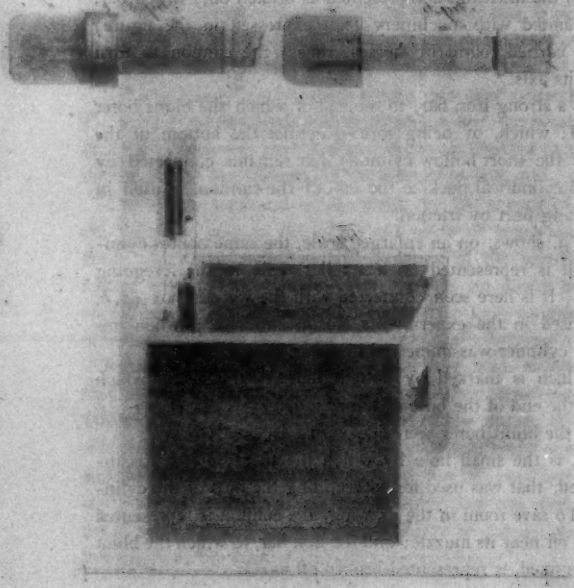
n is the blunt borer seen side-wise.

d, *e*, is the small hole by which the thermometer was introduced, that was used for ascertaining the heat of the cylinder. To save room in the drawing, the cannon is represented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is represented broken off at *m*.

Fig. 4. is a perspective view of the wooden box, a section of which is seen in the foregoing figure, (see *g*, *b*, *i*, *k*, fig. 3.)

Fig. 5 and 6. represent the blunt borer *n*, joined to the iron bar *m*, to which it was fastened.

Fig. 7 and 8. represent the same borer, with its iron bar, together with the piston which, in the experiments No. 2 and No. 3, was used to close the mouth of the hollow cylinder.



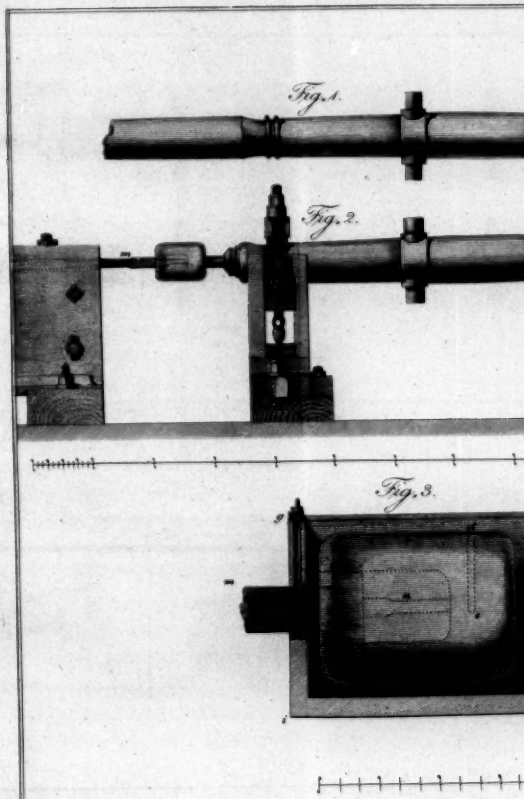


Fig. 1.
Fig. 2.
Fig. 3.

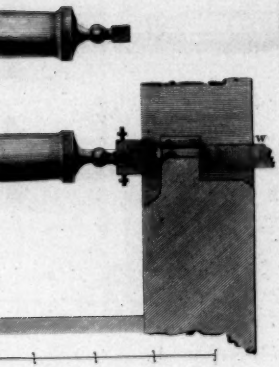


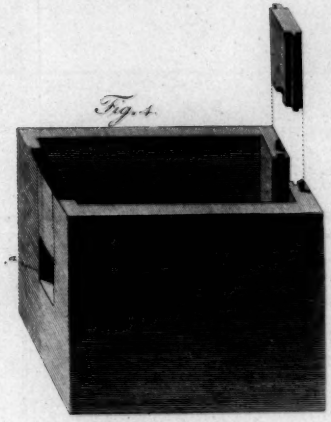
Fig. 5.

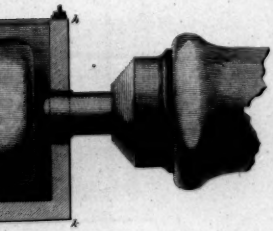
Fig. 7.

Fig. 6.

Fig. 8.

Fig. 4.





V. *Observations on the* Foramina Thebesii *of the Heart. By Mr.* John Abernethy, *F. R. S. Communicated by* Everard Home, *Esq. F. R. S.*

Read February 1, 1798.

As the investigation of the resources of nature in the animal œconomy, for the maintenance of health, and the prevention of disease, cannot but be interesting to the philosopher as well as to the physician, I therefore am induced to submit to the Society the following observations.

There is a remarkable contrivance in the blood vessels which supply the heart, not to be met with in any other part of the body, and which is of great use in the healthy functions of that organ, but which is particularly serviceable in preventing disease of a part so essential to life.

A distended state of the blood vessels must always impede their functions, and consequently be very detrimental to the health of the part which they supply; but, as the cavities of the heart are naturally receptacles of blood, a singular opportunity is afforded to its nutrient vessels, to relieve themselves when surcharged, by pouring a part of their contents into those cavities. Such appears to be the use of the foramina by which injections, thrown into the blood vessels of the heart, escape into the cavities of that organ; and which were first noticed by Vieussens, but, being more expressly described by Thebesius, generally bear the name of the latter author.

Anatomists appear to have been much perplexed concerning these *foramina Thebesii*; even HALLER, SENAC, and ZINN, were sometimes unable to discover them; which suggested an idea, that when an injection was effused into the cavities of the heart, the vessels were torn, and that it did not escape through natural openings. When these foramina were injected, they were found under various circumstances, as to their size and situation; and HALLER observed, that the injection, for the most part, escaped into the right cavities of the heart. It also remains undetermined, whether these foramina belong both to the arteries and veins, or respectively to each set of vessels.

It is from an examination of these openings in diseased subjects, that a solution of such difficulties may probably be obtained. Whoever reflects on the circumstances under which the principal coronary vein terminates in the right auricle of the heart, will perceive that an impediment to the flow of blood through that vessel must occasionally take place; but the difficulty will be much increased, when the right side of the heart is more than ordinarily distended, in consequence of obstruction to the pulmonary circulation. Indeed it seems probable, that such an obstruction, by occasioning a distended state of the right side of the heart, and thus impeding the circulation in the nutrient vessels of that organ, would as necessarily occasion corresponding disease in it, as an obstruction to the circulation in the liver occasions disease in the other abdominal viscera, were it not for some preventing circumstances, which I now proceed to explain.

Having been attentive to some very bad cases of pulmonary consumption, from a desire to witness the effects of breathing medicated air in that complaint, I was led to a more particular

examination of the heart of those patients who died. In these cases, I found, that by throwing common coarse waxen injection into the arteries and veins of the heart, it readily flowed into the cavities of that organ; and that the left ventricle was injected in the first place, and most completely. When the ventricle was opened, and the effused injection removed, the foramina Thebesii appeared both numerous and large, and distended with the different coloured wax which had been impelled into the coronary arteries and veins. Upon eight comparative trials, made by injecting the vessels of hearts taken from subjects whose lungs were either much diseased, or in a perfectly sound state, I found, that in the former, common injection readily flowed, in the manner which I have described, into all the cavities of the heart, but principally into the left ventricle; whilst, in many of the latter, I could not impel the least quantity of such coarse injection into that cavity.

This difference in the facility with which the cavities of the heart can be injected from its nutrient vessels, was observed by most anatomists, though they did not advert to the circumstances on which it depended. HALLER's recital of his own observations, and of those of others on this subject, so well explain the facts which I have stated, that I shall take the liberty of quoting the passage, in order further to illustrate and authenticate them. He says, "Si per arterias liquorem in-"jeceris, perinde in dextra auricula, sinuque et ventriculo dextro, "et in sinu atque thalamo sinistro guttulæ exstillabunt; sæpe "quidem absque mora, alias difficilius, et nonnunquam omnino, "uti continuo dicemus, et mihi, et SENACO, et clarissimo ZINNIO, "nihil exsudavit."—*Elem. Physiol. Tom.* I. *page* 382.

As it seems right that the blood which had been distributed

by the coronary arteries, and which must have lost, in a greater or less degree, the properties of arterial blood, should not be mixed with the arterial blood which is to be distributed to every part of the body, but ought rather to be sent again to the lungs, in order that it may re-acquire those properties; we therefore perceive why, in a natural state of the heart, the principal foramina Thebesii are to be found in the *right* cavities of that organ. However, as, even in a state of health, those cavities are liable to be uncommonly distended, in consequence of muscular exertion sometimes forcing the venous blood into the heart faster than it can be transmitted through the lungs, there seems to arise a necessity for similar openings on the left side; but these, in their natural state, though capable of emitting blood, and of relieving the plethora of the coronary vessels, are not of sufficient size to give passage to common waxen injections. Yet, when there is a distended state of the right cavities of the heart, which is almost certainly occasioned by a diseased state of the lungs, these foramina leading into the left cavities then become enlarged, in the manner that has been already described; and thus the plethoric state of the nutrient vessels of the heart, and the consequent disease of that important organ, are prevented.

The preceding remarks will, I think, sufficiently explain the cause of the variety in the size and situation of these foramina, which also appear to belong both to the arteries and veins; because, the injection which was employed was too coarse to pass from one set of vessels to the other, and yet the different coloured injections passed into the cavities of the heart unmixed.

There is yet another mode by which diseases of the heart, that would otherwise so inevitably succeed to obstruction in the

pulmonary vessels, are avoided; and which I next beg leave to explain.

Having formerly been much surprised to find the heart so little affected, when the lungs were greatly diseased, and observing, in one or two instances, that the *foramen ovale* was open, I was led to pay more particular attention to the state of that part; and I have found this to be almost a constant occurrence in those subjects where pulmonary consumption had for some time existed previous to the person's decease. I took notice of this circumstance thirteen times in the course of one year; and, in several instances, the aperture was sufficiently large to admit of a finger being passed through it. Now, as the *septum auricularum* is almost constantly perfect in subjects whose lungs are healthy, I cannot but conclude, that the renewal of the *foramen ovale* is the effect of disease: nor will the opinion appear, on reflection, improbable; for the opening becomes closed by the membranous fold growing from one edge of it, till it overlaps the other, and their smooth surfaces being kept in close contact, by the pressure of the blood in the left auricle, they gradually grow together. But, should there be a deficiency of blood in the left auricle, and a redundance in the right, the pressure of the latter on this membranous partition, will so stretch and irritate the uniting medium, as to occasion its removal; and thus a renewal of the communication between the auricles will again take place.

From these observations it is natural to suppose, that in those men, or animals, who are accustomed to remain long under water, this opening will either be maintained or renewed: yet on this circumstance alone the continuance of their life does not depend; for we now have sufficient proof, that if the

blood is not oxygenated in the lungs, it is unfit to support the animal powers. There is an experiment related by BUFFON, the truth of which, I believe, has not been publicly controverted, and which tends greatly to misrepresent this subject. He says, that he caused a bitch to bring forth her puppies under warm water; that he suddenly removed them into a pail of warm milk; that he kept them immersed in the milk for more than half an hour; and that when they were taken out of it, all the three were alive. He then allowed them to respire about half an hour, and again immersed them in the warm milk, where they remained another half hour; and, when taken out, two were vigorous, but the third seemed to languish: this submersion was again repeated, without apparent injury to the animals.

This experiment is so directly contrary to what we are led to believe from all others, and also to the information derived from cases which frequently occur in the practice of midwifery, (in which, an interruption to the circulation through the umbilical chord occasions the death of the fœtus,) as to make me suspect its truth: I was therefore induced to examine what would happen in a similar experiment. I did not indeed cause the bitch to bring forth her puppies in water; but immersed a puppy, shortly after its birth, under water which was of the animal temperature. It lost all power of supporting itself in about 60 seconds, and would shortly have perished, had I not removed it into the air. Neither could I, by repeating this experiment, so accustom the animal to the circulation of unoxygenated blood, as to lengthen the term of its existence in such an unnatural situation. I thought that a dog might have been made a good diver in this way; but, having satisfied myself

that this could not be done, without greatly torturing the animal, I did not choose to prosecute so cruel an experiment.

Young animals, indeed, retain their irritability for a considerable time, so that they move long after they have been plunged beneath water; and may even, on this account, recover after they are taken out. But the manner in which BUFFON has related his experiment seems to imply, that the circulation of the blood, and other functions of life, were continued after the animals had been excluded from the air. I am convinced that the poor dog who was the subject of my experiment would have been beyond recovery in a few minutes.

Those animals who are accustomed to remain long under water, probably first fill their lungs with air, which may, in a partial manner, oxygenate their blood during their submersion. The true statement of this subject may probably be, that the circulation of venous blood will destroy most animals in a very short space of time; but that custom may enable others to endure it, with very little change, for a longer period.

VI. *An Analysis of the earthy Substance from New South Wales, called* Sydneia *or* Terra Australis. *By* Charles Hatchett, *Esq. F. R. S.*

Read February 8, 1798.

§. 1.

THE late ingenious JOSIAH WEDGWOOD, Esq. F. R. S. published, in the Philosophical Transactions for the year 1790, an account of some analytical experiments on a mineral substance from Sydney Cove, in New South Wales.*

This substance, Mr. WEDGWOOD describes to be composed of a fine white sand, a soft white earth, some colourless micaceous particles, and also some which were black, resembling black mica, or black lead.

Nitric acid did not appear to act on any part of this earthy substance; and even a portion on which sulphuric acid had been boiled to dryness, afforded afterwards, when edulcorated with water, only a few flocculi, which Mr. WEDGWOOD conceived to be aluminous earth.

The muriatic acid, during digestion, seemed to act as little as the two preceding acids; but, upon water being poured in, to wash out the remaining portion, the liquor instantly became white as milk, with a fine white curdy substance intermixed; the concentrated acid having, in the opinion of the author,

* Philosophical Transactions, Vol. LXXX. Part II. page 306.

extracted something which the simple dilution with water precipitated.

The remaining part was repeatedly digested with muriatic acid, and treated with water, as before, till the milky appearance was no longer produced.

The properties of this white precipitate, Mr. WEDGWOOD states to be as follows.

1st. It is only soluble in boiling concentrated muriatic acid.

2dly. It is precipitated by water, in the form of a white earth; which may again be dissolved by boiling muriatic acid.

3dly. When nitric acid is mixed with the muriatic solution of this earth, there is no appearance of a precipitate; not even when water is added, provided that the nitric acid exceeds, or nearly approaches, the quantity of muriatic acid.

4thly. The earth is precipitated by the alkalies.

5thly. The muriatic solution does not crystallize by evaporation; but becomes a butyraceous mass, which soon liquefies on exposure to the air.

6thly. The butyraceous mass is not corrosive to the taste; and is even less pungent than the combination of calcareous earth with the same acid.

7thly. Heat approaching to ignition disengages the acid from the butyraceous mass, in white fumes, and a white substance remains.

8thly. The white precipitated earth is fusible *per se*, in from 142° to 156° of Mr. WEDGWOOD's thermometer, and it is thus distinguished from all the other primitive earths.

And, 9thly. This precipitate cannot be reduced to a metallic state, when exposed to heat with inflammable substances.

From these properties, Mr. WEDGWOOD says, that although

he cannot absolutely determine whether this substance belongs to the class of earths, or that of metallic substances, yet he is inclined to refer it to the former.

Professor BLUMENBACH, of Gottingen, in his Manual of Natural History, published in 1791, also mentions that he had examined a portion of this earthy substance, by means of muriatic acid, after the manner of Mr. WEDGWOOD, and that he had obtained a slight precipitate by the addition of water.*

In consequence of these experiments, the mineralogists throughout Europe admitted the white precipitated substance to be a primitive earth; and we accordingly find, in all the systematical works on mineralogy published since the above-mentioned period, that it is arranged as a distinct genus, under the names of *Sydneia*, *Australa*, *Terra Australis*, and *Austral Sand*.

The extreme scarcity of this substance prevented the chemists in general from examining more minutely into the nature of this new primitive earth, till Mr. KLAPROTH, in the second volume of his Additions to the Chemical Knowledge of Mineral Bodies, gave to the public a memoir entitled, A Chemical Examination of the *Austral Sand*.†

In this memoir, Mr. KLAPROTH says, that he had received from Mr. HAIDINGER, of Vienna, two samples of this substance; one of which had a considerable quantity of black shining particles intermixed with it, which, although regarded by many as graphite or plumbago, he was inclined to believe to be *Eisenglimmer* or micaceous iron ore.

The other contained much less of these black or dark grey

* *Handbuch der Naturgeschichte*, p. 567 and 568.

† *Beiträge zur Chemischen Kenntniss der Mineralkörper*.—*Zweiter Band*, p. 66.

particles, and, as he considered it to be more pure than the former, he subjected it to the following experiments.

1. It was digested at three different times with concentrated muriatic acid, in a boiling heat, and the acid was afterwards filtrated through paper. The solution was then mixed by degrees with pure water, which did not however produce any precipitate, even when warmed.

Carbonate of potash caused some flocculi to fall, which, edulcorated and dried, weighed 3.25 grains.

This precipitate was dissolved in diluted sulphuric acid, and left a small portion of siliceous earth; after which the solution, by evaporation, afforded crystals of alum.

2. The residuum of the muriatic solution was mixed with three times the weight of potash, and exposed to a red heat. Muriatic acid was then poured on the mass, and the insoluble gelatinous residuum was edulcorated on a filter; and, after a red heat, weighed 19.50 grains, which proved to be siliceous earth.

3. The muriatic solution, with prussiate of potash, afforded a blue precipitate; the ferruginous part of which was about one quarter of a grain.

4. The solution was then saturated with carbonate of potash, and some alumine was precipitated; which, after a red heat, weighed 8.50 grains, and with sulphuric acid formed alum.

Siliceous earth, alumine, and iron, appeared therefore to be the only ingredients of this substance; but, as Mr. KLAPROTH had no more than thirty grains to examine, he could not extend his experiments.

From those above related, he is of opinion that the existence

of this primitive earth may be much doubted, and that this doubt can only be removed in the course of time, by other analyses.

Mr. KLAPROTH concludes his memoir by saying, that the substance examined by him was undoubtedly the genuine austral sand, as Mr. HAIDINGER had received it from Sir JOSEPH BANKS, when he was in London.

Mr. NICHOLSON, however, in the 9th Number of his Journal of Natural Philosophy, *&c.* (p. 410.) published on the 1st of December, 1797, questions much, whether the substance examined by Mr. KLAPROTH was the same as that examined by Mr. WEDGWOOD; and, after having contrasted their experiments, says, "hence it seems fair to conclude that the two "minerals were not the same, however this may have hap-"pened; and that the existence of the new fusible earth of "WEDGWOOD stands on the same evidence as before, namely, "his experiments, which have not yet been repeated, that I "know of."

Some of Mr. NICHOLSON's objections to the experiments of Mr. KLAPROTH, being founded principally on some difference in the external characters of the substance examined by him, and the one examined by Mr. WEDGWOOD, are such as very naturally occur; but the following pages will, I believe, prove that Mr. KLAPROTH's experiments were made on that which might be justly regarded as the *Sydneia* or austral sand.

In 1796, the Right Hon. Sir JOSEPH BANKS, P. R. S. favoured me with a specimen of the *Sydneia*, which had been lately brought to England; a portion of this I soon after examined, in a cursory manner, by muriatic acid, but did not obtain any precipitate when water was added to the filtrated solution.

Upon mentioning this circumstance, and expressing a desire to examine this substance with more accuracy, Sir JOSEPH BANKS, with his usual readiness to promote every scientific inquiry, not only permitted me to take specimens from different parts of the box which contained the earth already mentioned, but (that every doubt might be obviated) gave me about 300 grains which remained of the identical substance examined by Mr. WEDGWOOD.

Upon these the following experiments were made; and, to distinguish them, I shall call the first, No. 1, and that examined by Mr. WEDGWOOD, No. 2.

§. 2.

Analysis of the Sydneia, *No.* 1.

The *Sydneia*, No. 1, is in masses and lumps, of a pale greyish white, intermixed with a few particles of white mica, and also occasionally with some which are of a dark grey, resembling graphite or plumbago.

It easily crumbles between the fingers, to a powder nearly impalpable, which has rather an unctuous feel.

Small fragments of vegetable matter are also commonly found intermixed with it; and the general aspect is that of an earthy substance which has been deposited by water.

EXPERIMENT 1.

400 Grains were put into a glass matrass, and one quart of distilled water being added, the whole was boiled to one fourth.

The liquor was then filtrated, and a portion being examined by the re-agents commonly used, afforded no trace of matter

in solution. The remainder was then evaporated, without leaving any residuum.

EXPERIMENT 2.

About 200 grains of the earth, rubbed to a fine powder, were put into a glass retort, into which I poured three ounces of concentrated pure muriatic acid. The retort was placed in sand, and the acid was distilled, till the matter in the retort remained dry. Two ounces of muriatic acid were again poured on it, and distilled as before, till only one fourth remained. The whole was then put into a matrass, which was placed in an inclined position, so that when the earth had subsided, the liquor might be decanted, without disturbing the sediment.

When it had remained thus for 12 hours, the acid was carefully poured into a glass vessel; but, as I observed that it was not so perfectly transparent as before it had been thus employed, I suffered it to remain 24 hours, but did not perceive any sediment. Half of this liquor was diluted with about twelve parts of distilled water, and, after a few hours, a very small quantity of a white earth subsided.

This however did not appear to me to be a precipitate caused by a change in the chemical affinities, but rather an earthy matter which had been suspended in the concentrated acid, and afterwards deposited, when the liquor was rendered less dense by the addition of water. To ascertain this, I poured the remaining portion of the concentrated liquor on a filter of four folds: it passed perfectly transparent, and, although diluted with twenty-four parts of water, it remained unchanged, and as pellucid as before. I now filtrated the former portion, and added it to that already mentioned.

It was then evaporated to dryness, and left a pale brownish mass, which was dissolved again, by digestion, in the smallest possible quantity of muriatic acid.

Water was added, in a very large proportion, to this solution, without producing any effect; I then, by prussiate of potash, precipitated a quantity of iron, which was separated by a filter.

The clear solution was then saturated with lixivium of carbonate of potash, and a white precipitate was produced, which was collected and edulcorated. This, when digested with diluted sulphuric acid, was dissolved; and the superfluous acid being driven off by heat, boiling water was poured on the residuum, and completely dissolved it.

To this solution some drops of lixivium of potash were added, and, by repeated evaporations, the whole formed crystals of alum.

From the above experiment it appeared, that the muriatic acid had only dissolved some alumine and iron; but, in order to satisfy myself more completely in respect to the component parts of this substance, I made the following analysis.

Analysis. A. 400 grains were put into a glass retort, which was then made red-hot during half an hour. Some water came over, and the earth afterwards weighed 380.80 grains, so that the loss amounted to 19.20 grains. The greater part of this loss was occasioned by the dissipation of the water imbibed by the earth; to which must be added, the loss of weight caused by the combustion of a small portion of vegetable matter.

B. The 380.80 grains were rubbed to a fine powder, and being put into a glass retort, 1470 grains of pure concentrated sulphuric acid were added. The retort was then placed in a small reverberatory, and the fire was gradually increased, till

the acid was distilled over: it was then poured back on the matter in the retort, and distilled as before, till a mass nearly dry remained.

On this, boiling distilled water was repeatedly poured, until it no longer changed the colour of litmus paper, and was devoid of taste. The undissolved portion was then dried, and made red-hot; after which it weighed 281 grains.

C. I now mixed the 281 grains with 300 grains of dry carbonate of potash, and exposed the mixture to a strong red heat, in a silver crucible, during four hours. The mass was loose, and of a greyish white: it was softened with water, and, being put into a retort, sulphuric acid was added to a considerable excess. The whole was then distilled to dryness, and, when a sufficient quantity of boiling water had been added, it was poured on a filter, and the residuum was well washed; it was then made red-hot, and afterwards weighed 274.75 grains.

D. The solutions of B and C were added together, and were much reduced by evaporation. Pure ammoniac was then employed to saturate the acid, and a copious loose precipitate, of a pale yellowish colour was produced; which, collected, edulcorated, and made red-hot, weighed 103.70 grains.

E. The filtrated liquor of D was again evaporated, and carbonate of potash being added, a slight precipitation of earthy matter took place; which, by the test of sulphuric acid, proved to be some alumine which had not been precipitated in the former experiment: this weighed 1.20 grain.

F. The 103.70 grains of D were completely dissolved when digested with nitric acid, excepting a small residuum of siliceous earth, which weighed 0.90 grain.

G. The nitric solution was evaporated to dryness, and a

second portion of the same acid was added, and in like manner evaporated. The residuum was then made red-hot, and digested with diluted nitric acid, which left a considerable portion of red oxide of iron. The solution was again evaporated, and the residuum, being treated as before, again deposited some oxide of iron, much less in quantity than the former.

The whole of the oxide was then heated with wax in a porcelain crucible, was taken up by a magnet, and weighed 26.50 grains.

H. The nitric solution of G was saturated with ammoniac, and a loose white precipitate was formed; which, edulcorated and made red-hot, weighed 76 grains.

I. These 76 grains were dissolved when digested with diluted sulphuric acid; and, when the excess of acid had been expelled by heat, the saline mass was dissolved in boiling water. To this solution I added some lixivium of potash, and, by gradual and repeated evaporations, obtained the whole in regular octoedral crystals of alum.

K. The 274.75 grains of C now alone remained to be examined. They appeared to consist of siliceous earth, mixed with the dark grey shining particles already mentioned; but, as I shall describe, in the following experiments, the process by which these were separated, I shall now only say that they amounted to 7.50 grains.

L. The earth with which the abovementioned particles were mixed weighed 267.25 grains. This earth was white, and arid to the touch: when melted with two parts of soda, it formed a colourless glass; and, with four parts of the same it dissolved in water, and formed a *liquor silicum*: it was therefore pure siliceous earth or silica.

The substance here examined was composed therefore of the following ingredients.

		grains.
Pure siliceous earth or silica -	F.	0.90
	L.	267.25
Alumine - - - -	E.	1.20
	H.	76
Oxide of iron - - -	G.	26.50
Dark grey particles - -	K.	7.50
Water and vegetable matter -	A.	19.20
		398.55

The foregoing analysis was repeated several times, and always with similar results; excepting, that as I had taken the specimens from different parts of a large quantity, I found that the proportions of the ingredients were not constantly the same: that of the siliceous earth, for example, was sometimes greater, and the alumine and iron proportionably less. Some specimens were also nearly or totally destitute of the dark grey shining particles; in short, every circumstance was such as might be expected from a mixed substance, which, from the nature of its formation, cannot have the ingredients in any fixed proportion.*

As this substance agreed in its general characters, for the greater part, with that described by Mr. WEDGWOOD, and as it was indisputably brought from the same place, there appeared every reason to believe that the nature of both was the same;

* The description given by Mr. KLAPROTH convinces me, that his experiments were made on a portion of this substance. Moreover, when my late friend Mr. HAIDINGER was in London, I gave him some of this earth for his collection; so that, whether Mr. KLAPROTH made his experiments on that which had been received by Mr. HAIDINGER from Sir JOSEPH BANKS, or from myself, it is not less certain that he operated on that which might be regarded as the genuine *Sydneia*.

but, to obviate as much as possible any doubt or objection, I determined to repeat the experiments, and the analysis, on that portion which remained of the identical substance examined by Mr. WEDGWOOD, and which from that period had been reserved by Sir JOSEPH BANKS, who kindly favoured me with it for this purpose.

§. 3.

Analysis of the Sydneia, *No.* 2.

This substance, as has already been mentioned, consists of a white transparent quartzose sand, a soft opaque white earth, some particles of white mica, and a quantity of dark lead-grey particles, which have a metallic lustre.

The Sydneia, No. 2, appears chiefly to differ from No. 1, by being more arenaceous, and by a larger proportion of the dark grey particles. Many experiments, similar to those made on No. 1, already described, were made on this substance, with pure concentrated muriatic acid; but, as none of these afforded any appearance of a precipitate by the means of water, I do not think it necessary to enter into a circumstantial account of them, and shall proceed therefore to the analysis.

A. 100 grains were exposed to a red heat, in a glass retort, and, after half an hour, were found to have lost in weight 2.20 grains.

B. The 97.80 grains which remained were mixed with 300 grains of dry carbonate of potash, and the mixture was exposed to a strong red heat, in a crucible of silver, during three hours.

When cold, the mass was softened with water, and was put into a glass matrass. I then added three ounces of pure con-

centrated muriatic acid, and digested it for two hours in a strong sand heat. Boiling water was then added, and the whole being poured on a filter, the residuum was edulcorated, dried, and made red-hot; it then weighed 85.50 grains.

C. The filtrated solution was evaporated to one fourth, and pure ammoniac being added, a precipitate was formed, which, after a red heat, weighed 10.70 grains.

D. One ounce of muriatic acid was poured on the 10.70 grains, in a matrass, which was then heated. The whole of the 10.70 grains was dissolved, excepting a small portion of siliceous earth, which weighed 0.30 grain.

E. The muriatic solution was then reduced by evaporation, to about one fourth; to which I added a large quantity of distilled water, which did not however produce any change. I then gradually added a solution of pure crystallized prussiate of potash, and heated the liquor till the whole of the iron was precipitated; after which, ammoniac precipitated a loose white earth, which, edulcorated and made red-hot, weighed 7.20 grains. The iron precipitated by the prussiate may therefore be estimated at 3.20 grains.

F. The 7.20 grains of the white earth were digested with sulphuric acid, and, after the excess of acid had been expelled by heat, boiling water was poured on the saline residuum. The solution was then gradually evaporated, with the addition of a small portion of lixivium of potash, and afforded crystals of alum, without a trace of any other substance.

G. I now proceeded to examine the 85.50 grains of B. These appeared to consist of siliceous earth, or fine particles of quartz, mingled with a considerable quantity of the dark grey shining particles.

Mr. WEDGWOOD was of opinion that these were a peculiar species of plumbago or graphite. Professor BLUMENBACH, on the contrary, regards them as molybdæna: and Mr. KLAPROTH believes them to be *eisenglimmer* or micaceous iron ore.

When rubbed between the fingers, they leave a dark grey stain, and the feel is unctuous, like that of plumbago, or molybdæna: the traces which they make on paper also resemble those of the abovementioned substances, but the lustre of the particles approaches nearer to that of molybdæna.

In order therefore to determine whether or not they consisted totally or partially of molybdæna, I put the 85.50 grains into a small glass retort, and added two ounces of concentrated nitric acid. The retort was then placed in a sand heat, and the distillation was continued, till the matter remained dry. The acid was then poured back into the retort, and distilled as before; but I did not observe that the grey particles had suffered any change, nor were nitrous fumes produced, as when molybdæna is thus treated.

To be more certain, however, I digested pure ammoniac on the residuum; and, having decanted it into a matrass, I evaporated it to dryness, without perceiving any vestige of oxide of molybdæna, or indeed of any other substance.

It was evident therefore that molybdæna was not present; and, as the general external characters and properties corresponded with those of plumbago, I was inclined to believe that these were particles of that substance, and not micaceous iron, as Mr. KLAPROTH imagined. To determine this, the following experiment was made.

H. 200 grains of pure nitre in powder were mixed with the 85.50 grains, and the mixture was gradually projected into a

crucible, made strongly red-hot. A feeble detonation took place at each projection; and, after a quarter of an hour had elapsed, the crucible was removed.

When cold, the mass was porous and white, without any appearance of the dark grey particles. Boiling water was poured on it, and the whole being put into a matrass, one ounce of muriatic acid was added, and digested with it in a sand heat. By evaporation it became gelatinous: it was then emptied on a filter, and, being well washed, dried, and made red-hot, weighed 75.25 grains.

The appearance of this was that of a white earth, arid to the touch. When melted with two parts of soda, a colourless glass was formed; and, with four parts of the same, it was soluble in water, and produced *liquor silicum*; it was therefore pure siliceous earth.

I. The filtrated liquor was saturated with ammoniac, and, upon being heated, a few brownish flocculi were precipitated, which, when collected and dried, weighed 0.40 grain. This precipitate was dissolved in muriatic acid, and was again precipitated by prussiate of potash, in the state of Prussian blue.

The liquor from which the flocculi of iron had been separated was then examined, by adding carbonate of potash, and lastly, by being evaporated to dryness; but it no longer afforded any earthy or metallic substance: so that, by the process of detonation with nitre, the 85.50 grains afforded 75.25 grains of pure siliceous earth, with 0.40 grain of iron; and, as the dark grey substance was destroyed, excepting the 0.40 grain of iron abovementioned, and as 9.85 grains of the original weight of 85.50 grains were dissipated, there can be no doubt but that this substance, amounting to 10.25 grains, was carburet of iron or

plumbago; especially as some experiments which I purposely made, on that from Keswick in Cumberland, were attended with similar results.

It is also evident, that these particles could not be *eisenglimmer* or micaceous iron, as nitre has little or no effect on that substance, when projected into a heated crucible.

In a subsequent experiment on the same, the crucible was removed immediately after the last projection, and I then observed that an effervescence, with a disengagement of carbonic acid, took place, upon the addition of the muriatic acid, as is usual when pure plumbago is decomposed by nitre, and that less of the gelatinous matter was formed by evaporation.

The cause of this difference was evidently the duration of the red heat; for, in the first instance, the alkali developed by the decomposition of the nitre had time to unite with the siliceous earth, so as, when dissolved, to form *liquor silicum*; but, in the second experiment, a portion of alkali remained combined with the carbonic acid, produced by the carbon of the decomposed plumbago.

The produce of 100 grains by this analysis was,

		grains.
Silica - - -	D.	0.80
	H.	75.25
Alumine - - -	F.	7.20
Oxide of iron - -	E.	3.20
Graphite or plumbago -	I.	10.25
Water - - -	A.	2.20
		98.40

Mr. WEDGWOOD says, that sulphuric acid cannot dissolve the precipitated earth, and has but little effect on the mixed

substance, even when distilled to dryness; but, from the preceding experiments, I had reason to believe that the aluminous earth and iron would be separated by reiterated distillation; I therefore repeated the analysis in the following manner.

Second Analysis of the Sydneia, *No.* 2.

A. 100 grains of the earth were put into a glass retort, upon which 400 grains of pure concentrated sulphuric acid were poured. The retort was placed in a small reverberatory, and the fire was continued till a dry mass remained. 400 grains of the acid were again poured in, and distilled as before. Upon the dry mass, boiling water was poured, and the whole was then emptied on a filter, and edulcorated. The residuum, after a red heat, weighed 87.75 grains, and consisted of siliceous earth, mixed with some mica, and with particles of plumbago.

B. The filtrated solution, by ammoniac, afforded a precipitate, which weighed 9.50 grains; and, being examined, as in the former experiment, yielded 6.50 grains of alumine, and 3 grains of oxide of iron.

The plumbago was separated from the siliceous matter, in the manner already described, and amounted to about 10 grains.

By this analysis I obtained,

	grains.
Silica and mica - - -	77.75
Alumine - - -	6.50
Oxide of iron - - -	3
Plumbago - - -	10
	97.25

It appears therefore that the Sydneian earth, when treated with sulphuric acid, is capable of being for the greater part

decomposed; and Mr. WEDGWOOD probably did not succeed, because his process was in some respect different, or that the distillation was not sufficiently repeated.

I have not thought it necessary to be more circumstantial in the account of this second analysis, as the operations were similar to those of the former.

§. 4.

These experiments prove, that the earthy substance called *Sydneia* or *terra australis*, consists of siliceous earth, alumine, oxide of iron, and black lead or graphite.

The presence of the latter appears to be accidental, and it probably was mixed with the other substances at the time when they were transported, and deposited, by means of water; for this appears evidently to have been the case, from the general characters of this mixed earthy substance.

The quartz and mica, which are so visible, indicate a granitic origin; and the soft white earth has probably been formed by a decomposition of feldt spar, such as is to be seen in many places, and particularly at St. Stephen's, in Cornwall. The granitic sand which covers the borders of the *Mer de Glace*, at Chamouni, in Savoy, also much resembles the *terra australis*, excepting that the feldt spar is not in a state of decomposition: in short, the general aspect, and the analysis, concur to prove, that the *Sydneia* has been formed by the disintegration and decomposition of granite, or *gneiss*.

Mr. WEDGWOOD's experiments are so circumstantial, that had I only examined the earth last brought to England, I should have supposed, with Mr. NICHOLSON, that I had operated on a different substance; but, as I had an opportunity to examine,

by analysis, a portion of the same earth on which Mr. WEDGWOOD made his experiments, and as I received it from Sir JOSEPH BANKS, the same gentleman who had furnished Mr. WEDGWOOD with it, no suspicion can be entertained about its identity.

Some of the experiments which I have related, and which prove that some of the finer earthy particles remained suspended in the concentrated muriatic acid, and were precipitated when the acid was diluted with water, appear in some measure to account for the mistake which has been made, in supposing that a primitive earth, before unknown, was present; but this alone will not account for many of the other properties mentioned by Mr. WEDGWOOD, such as,

1st. The repeated and exclusive solubility in the muriatic acid, and subsequent precipitation by water.

2dly. The butyraceous mass which was formed by evaporation.

And, 3dly. The degree of fusibility of the precipitated earth.

These indeed I can by no means explain, but by supposing that the acids used by Mr. WEDGWOOD were impure. This supposition appears to be corroborated by a passage in Mr. WEDGWOOD's paper, where he says, "here the Prussian lixi-"vium, in whatever quantity it was added, occasioned no pre-"cipitation at all, (only the usual bluishness arising from the "iron always found in *the common acids.*")* Now if (as it seems from this expression) Mr. WEDGWOOD employed the common acids of the shops, without having previously examined and purified them, all certainty of analysis must fall, as the impurity of such acids is well known to every practical

* Philosophical Transactions, Vol. LXXX. Part II. p. 313.

chemist; but, whether this was the cause, or not, of the effects described by Mr. Wedgwood, I do not hesitate to assert, that the mineral which has been examined does not contain any primitive earth, or substance possessing the properties ascribed to it, and consequently, that the Sydneian genus, in future, must be omitted in the mineral system.

VII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, *for the Year* 1798. *By* Thomas Barker, *Esq. Communicated by Mr.* Timothy Lane, *F. R. S.*

Read February 15, 1798.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	29,77	28,47	29,17	50	41	45	52½	32	42½	1,955
	Aftern.				52	41½	46	55	34	47	
Feb.	Morn.	29,87	28,53	32	45½	38	42	49	31½	38	1,643
	Aftern.				51	34	42½	51½	32	43	
Mar.	Morn.	29,97	29,22	57	48	33	41	45	26	36	0,376
	Aftern.				51	34	42½	57	34	45	
Apr.	Morn.	29,88	28,67	59	59	44	50½	52	36	45	0,649
	Aftern.				61	45	52	72	45	57½	
May	Morn.	29,73	28,33	27	55	47	51	52	40	46	2,839
	Aftern.				56	48	52	65	48	56	
June	Morn.	29,84	29,05	47	59	50½	55	60	46	54	0,927
	Aftern.				70	52	57	80	56	67	
July	Morn.	29,73	28,91	32	67	50	58½	64½	49½	56½	5,646
	Aftern.				67½	54½	60½	77	59	68½	
Aug.	Morn.	29,95	29,23	60	65½	57	61	62½	49	56	1,120
	Aftern.				68½	58½	63	77	57	69	
Sep.	Morn.	29,83	29,03	50	65	53	59½	63	43½	54	1,891
	Aftern.				68	54	61	77	56	66	
Oct.	Morn.	30,07	28,75	45	55½	43½	49	59	29	43	1,320
	Aftern.				56	44½	50½	61½	41	51½	
Nov.	Morn.	29,86	28,69	36	51½	39½	43	52½	26	38	2,048
	Aftern.				53	39	43½	57	28½	43	
Dec.	Morn.	29,98	28,75	31	44½	28	34	48	14½	30	1,668
	Aftern.				46½	29½	35	49	25	34	
											22,082

THE year began with a remarkably open winter, sometimes quite warm and pleasant, and several times thunder. It was showery at first, then dry and fine; but the end of January and beginning of February were wet, yet still open and mild, and more dry afterward; but colder, and inclined to frost, the end of February, and in March, the middle of which was again mild and fine, and not windy, but frosty toward the end. The last days of March, and beginning of April, were dry and pleasant; a good seed time, and calm; but rain was wanted toward the end, which came plentifully the end of April, and beginning of May. The season in general cool, and the latter half more dry; too much so in the south of England, for grass and hay were scarce there; but, in this country, both grass and corn came on well, and continued to do so all June, which was of a moderate heat, with a mixture of wet and dry; frequent but moderate winds, and calm at the end. There was plenty of hay this year; but, through a very wet and windy July, a good deal of it was not well got. The crops of grain were almost all good, and the moist July made the beans and pease remarkably so. The harvest, though threatening at first, was in general very well got; the weather being chiefly fair, and rather hot, with some rain at times, kept the grass in a growing state, of which there was plenty left upon the ground against winter.

The autumn was in general very fine and pleasant; for the most part fair, with few frosty mornings, till near the end of November; when a severer season began, and continued very hard frost the first third of December; then an imperfect break for some days, but not so as to take the frost all away. It returned again as hard as before, and continued another third

of the month, till the two last days; when a thaw came on, with very warm and remarkably wet air, though no great quantity of rain fell. It was agreed every where, that December 24, at night, was much the coldest time this winter; in some places the thermometer was down below 0, at 4 or 8; but I did not happen to look at it just at that time, so that I never saw it so low as 14.

VIII. *An Account of some Endeavours to ascertain a Standard of Weight and Measure.* By Sir George Shuckburgh Evelyn, Bart. *F. R. S. and A. S.*

Read February 22, 1798.

§. 1.

HAVING for some years turned my thoughts to the consideration of an invariable and imperishable standard of weight and measure, as being a thing, in a philosophical view, highly desirable, and likely to become extremely beneficial to the public, I had, so early as the year 1780, taken up the idea of an universal measure, from whence all the rest might be derived, by means of a pendulum with a moveable centre of suspension, capable of such adjustments, as to be made to vibrate any number of times in a given interval; and, by comparison of the *difference* of the vibrations with the *difference* of the lengths of the pendulum, (which difference alone might be the standard measure,) to determine its positive length, if that should be thought preferable, under any given circumstances; by which means, all the difficulties arising in determining the actual centre of motion and of oscillation, which have hitherto so much embarrassed these experiments, would be gotten over.

(§. 2.) I made several computations of the probable accuracy that might be expected from such an experiment, and was satisfied with their result. But, not seeing clearly how such a pendulum could be connected to a piece of mechanism, to

number the vibrations without affecting them, I dropped the idea for that time. I learnt, however, some time afterwards, that Mr. JOHN WHITEHURST, a very ingenious person, had been in pursuit of the same object with better success, and had contrived a machine fully corresponding to his expectations and my wishes. This he afterwards explained to the world, in a pamphlet, entitled, "An Attempt to obtain Measures of Length, "&c. from the Mensuration of Time, or the true Length of "Pendulums;" published in 1787. Mr. WHITEHURST having therein done all that related to the standard measure of length, and suggested that of weight, it appeared to me that it remained only to verify and complete his experiments.

(§. 3.) For this purpose, by the kind assistance of my friend Dr. G. FORDYCE, who, at Mr. WHITEHURST's death, had purchased his apparatus, I was furnished with the very machine with which Mr. WHITEHURST had made his observations. I also procured to be made, by Mr. TROUGHTON, a very excellent beam-compass or divided scale, furnished with microscopes and micrometer, for the most exact observations of longitudinal measure: as also a very nice beam or hydrostatic balance, sensible with the $\frac{1}{100}$ of a grain, when loaded with 6lb. Troy at each end. Mr. ARNOLD made me one of his admirable time-keepers, in order to carry time from my sidereal regulator in my observatory, with which it was adjusted, to the room wherein I had fixed Mr. WHITEHURST's pendulum; and who, having taken a journey from London into Warwickshire, was so good as to assist in the beginning of these experiments. Thus equipped, I went to work in the latter end of August, 1796, when the temperature was about 60°, first to examine the length of the pendulum; when, to my great mortification, I found that the

thin wire, of which the rod consisted, was too weak to support the ball in a state of vibration; and that, after 15 or 20 hours action, it repeatedly broke. The same misfortune attended my trials with three other different sorts of wires that I had obtained from London. Whether this accident happened from any rust in the old wire, or from want of due temper in the new, or from its being too much pinched between the cheeks,* I cannot tell: I can only observe, that all the wires that I used were considerably heavier, and therefore probably stronger, than what Mr. Whitehurst mentions, *viz.* 3 grains in weight for 80 inches in length; nay, mine proceeded as far as from 5 to 6 grains for that length, and yet I could never get it to support the ball during the whole period of my experiment. This being the case, and being in the country, far removed from the manufactory of this fine wire, I was reluctantly compelled to relinquish this part of the operation to some more favourable opportunity. In the mean while, however, I thought it desirable to measure the difference of the lengths of Mr. Whitehurst's pendulum from his own observations; for, very fortunately, the marks that he had made on the brass vertical ruler of his machine were still visible; and this interval, which he calls "59,892 inches," I determined, on my divided scale made by Troughton, from Mr. Bird's standard, to be = 59,89358 inches, from a mean of four different trials in the temperature of 64°; that mean differing from the extremes only = ,0003 inch.

(§. 4.) By this examination, if I have not verified, I have at least preserved, Mr. Whitehurst's standard; and, for the present, I shall consider this measure of the difference of the length of the two pendulums, vibrating 42 and 84 times in a

* C, C, fig. 1. of Plate II. in Mr. Whitehurst's pamphlet.

minute of mean time, as correct. On this presumption, I shall proceed to the examination of weight.

(§. 5.) From the opinion of different skilful persons, with whom I have conferred, as well as from the result of my own considerations, I am inclined to believe there is hardly any body in nature, with which we are familiarly acquainted, that is of so simple and homogeneous a quality as pure distilled water, or so fit for the purposes of this inquiry; and I have concluded, that if the weight of any quantity of water, whose bulk had been previously measured by the abovementioned scale, could be obtained, under a known pressure* and temperature of the atmosphere, we should be in possession of a general standard of weight.

(§. 6.) With this view, I directed Mr. TROUGHTON to make, in addition to the very sensible hydrostatic balance before mentioned, a solid cube of brass, whose sides were 5 inches; and also a cylinder of the same metal, 4 inches in diameter, and 6 high. From St. Thomas's hospital, by the favour of Dr. FORDYCE, I procured 3 gallons of distilled water. With these I made the following observations; but, before I relate the experiments, I will describe the apparatus.

Mr. WHITEHURST'S machine for measuring the pendulum has been sufficiently explained in his pamphlet mentioned above; my divided scale, which was a new instrument, was as follows.

* I do not here mean to infer any opinion respecting the compressibility of water; but only to say, that where water, or any thing else, is weighed in air, the density of that medium, as shewn by the barometer and thermometer, must be known, in order to make allowances for it, if necessary.

(§. 7.) *Description of the Beam Compass, or divided Scale of equal Parts.*

a b, (Tab. V. fig. 1.) is a block or beam of mahogany, 6 feet 3 inches long, 6 inches deep, and 5 wide, upon which are laid two brass rulers, *c d e*, and *f g*, each divided into 60 inches, and tenths. The former of these, called the Scale, is, for a time, kept immoveable by the finger-screws *c e d*, and is furnished with very fine hair-line divisions, intended to be viewed only by the microscopes *b*, *i*: the latter, called the Beam, has no motion but by means of the screw *g*, and bears stronger divisions upon it, with which the sliding pieces or indexes, at *k* and *m*, may readily be compared by the naked eye, and is intended only to set the microscopes, or rather the wires, in their focus, to the required distance nearly, *viz.* to within $\frac{1}{100}$ or $\frac{1}{200}$ of an inch. The microscopes are compound, and similar to those described by the late General Roy, in his account of his large theodolite. (See Phil. Trans. Vol. LXXX.) The one at *b*, contains only cross wires fixed in its focus; the other at *i*, has a micrometer also, by means of which its cross wires may be moved to the right or left, over the image of the divisions of the scale, any given space, not exceeding $\frac{1}{10}$ inch; and the quantity so moved may be measured by the divisions on the screw head, passing under the index at *o*. The divisions on these rules have been called inches and tenths: it was not necessary that they should be more than equal parts; but they were in fact laid down by Mr. Troughton, from a scale of the late excellent artist Mr. J. Bird, who had divided into inches several scales of different lengths; one of which, 42 inches long, belonged to the late General Roy; a second, of 5 feet, was purchased by Alexander Aubert, Esq. and a third, of

90 inches, which is now the property of the Royal Society, is kept in their archives, and is said to have been used by Mr. BIRD, in dividing his large mural quadrants.* Besides these, he made two standards of three feet, by order of the House of Commons, of which I shall speak more hereafter. The mode of using this instrument is as follows.

(§. 8.) Let the object to be measured be supposed to be about six inches, and let it be desired to compare it with the interval between the 20th and the 26th division on the scale *c d*: move by hand the microscope *b*, with its sliding plate, until the division of the index at *k* coincide with the division of 20 inches on the rule *f g*; then move by hand also the microscope *i*, with its sliding plate and appendage *l m n o*, until the index division near *m* coincide with 26 inches on *f g*: the axes of the microscopes, or centres of their cross wires, will be at the approximate distance of 6 inches. To correct this, examine if the wires of *b* correspond with a division on *c d*; if not, move the rule *f g* backward or forward, by the screw *g*, till they do, then will the microscope *b* be adjusted. Now examine if the wires in *i* cover exactly a division; if they do so, the true interval of 6 inches between the microscopes is obtained; if not, move the microscope *i* a little, by means of the screw *l*, till they do, and both the microscopes will be adjusted: then remove the rule *c e d* from its place, by taking out the screws *c e d*, and place the object to be measured in its room, at the same time taking care that it be exactly in the focus of the object glass of the microscope, in such manner that one extremity may correspond with the wires in the microscope *b*; that done, if the other extremity coincide with the wires in *i*, the dimension of the object is exactly 6 inches; if not, restore the coincidence,

* A farther account of these scales is given in the Appendix.

by turning the micrometer screw *n*, and the divisions at *o* will give the difference, in 1000ths and 10,000ths of an inch, + or − 6 inches.

(§. 9.) *Description of the Hydrostatic Balance.*

a b c d, (Tab. VI.) is a box, which contains the whole apparatus when not in use, and when used serves as a foot to the hollow brass pillar *e f g h*, which is fixed into it by the four screws at the bottom *e* and *f*. This pillar contains another within it, which is raised up and down about $\frac{1}{10}$ inch, by means of the screw *x*. *n o* is the beam, 27 inches long, and 3,9 inches wide in its greatest diameter; each arm of which is made hollow and conical, for strength and lightness: through the centre, at *m*, passes the axis of motion, the ends of which, when used, are suffered to fall gently upon two crystal planes, which are set horizontally by means of the spirit levels *k*, *l*, and the screws underneath the box, at *c* and *b*. The ends of this axis are of hardened steel, of a wedge-like shape, and reduced to a fine edge, *viz.* to an angle of about 40°, so as to move upon the planes with very little friction, and at the same time so hard as (with due care in using) to be in no danger of being blunted: to prevent which, the inner pillar has a motion upwards, as has been said, by the screw *x*, and, by means of a semicircular arm at its upper extremity, lifts the beam off its bearings, when it is not used, or is greatly loaded. This axis is placed carefully at right angles to the beam; and, by means of two small brass springs that press gently at the ends, is brought always to have the same bearing upon the crystal; so that no error need be feared from a small deviation from the right-angular position of the axis to the beam, should any such

exist; and, from its shape and quality, it may be considered as inflexible in any ordinary experiments. At *p* is a small adjusting screw, which raises or depresses a weight within, and with it, in consequence, the centre of gravity of the whole beam; by this means, the motion on its centre may be brought to almost any required degree of sensibility. Should the centre of gravity be raised above the centre of motion, the beam would turn over; if it be in that centre, the beam would stand any where indifferently, without any vibration; if it be placed much below it, the vibration would be too quick, and its sensibility not sufficient: it is therefore brought, by the screw *p*, a very small quantity below the centre of motion, so as to describe one vibration in 40 or 50 seconds; the sensibility is then fully sufficient. At each end of the beam are circular boxes, *n* and *o*, through which pass the steel centres, from whence are suspended the scale-pans *q* and *r*: these centres resemble, in some degree, those at *m*, but have their chamfered or angular edges upwards, and thereon hang the hooks β, to which are affixed the links α, and to them the three silken lines of the scale. Each of these centres has a motion in its respective box, by means of two small adjusting screws; that in *o* laterally, and that in *n* vertically; the former to make the two arms of the beam of an equal length, the latter to bring the three points of suspension of the beam and scales into a right line. At the extremity of the boxes are fixed two needle points or indexes, which play against the ivory scale of divisions at *s* and *t*. These divisions, although they do not, indeed they cannot, shew any definite weight, are nevertheless very useful in making the adjustments, and even in weighing to the small fractions of a grain. *u v* are two steady plates, that are raised or depressed by the wooden nut

w, to check the vibrations of the scales *q* and *r*, and bring them more speedily to an equilibrium. *y z* is a table, whereon the whole is placed, to raise it to a height convenient for experiments.

To use with this beam, I had three sets of weights made, *viz.*

The 1st set or series of 15 weights, rising in a duplicate progression from 1 to 16384 grains, *viz.*

No.	Grains.	Fractions of a Grain.
1 =	1	
2 =	2	
3 =	4	
4 =	8	
5 =	16	$\frac{1}{32}$
6 =	32	$\frac{1}{16}$
7 =	64	$\frac{1}{8}$
8 =	128	$\frac{1}{4}$
9 =	256	$\frac{1}{2}$
10 =	512	
11 =	1024	
12 =	2048	
13 =	4096	
14 =	8192	
15 =	16384	

The 2d series of weights, in an arithmetical order, as follow, *viz.*

Grains.	Grains.	Grains.	Grains.	Decimal Fractions of a Grain, viz. 100th Grain.	Tenths.
1	10	100	1000	,01	,10
2	20	200	2000	,02	,20
3	30	...	....	,03	,30
4	40	400	4000	,04	,40
5	50	...	....	,05	,50
6	60	600	6000	,06	,60
7	70	...	....	,07	,70
8	80	800	8000	,08	,80
9	90	...	...	,09	,90
			10,000		
			20,000		

N. B. The fractions of a grain are made of fine wire flatted.

The 3d set consists of a weight of { 1 ounce, 2 ounces, 4 ounces, 8 ounces, 1 pound } Troy.

(§. 10.) *a b c d* and *a b c d*, (Tab. VII. fig. 1.) is the brass cube of 5 inches that has been mentioned, suspended in its own scale, by means of four fine wires, from the arm *o* of the beam, Tab. VI. by taking away the common scale *a r*. The cube rests upon a cradle or cross, three arms of which are seen at *g b i*, and by this means may be weighed either in air or water, by immersion into the large glass vessel *g b*, Tab. VII. fig. 3.

At fig. 2. is seen the cylinder *a b c d* and *a b c d*, four inches in diameter, and five high, slung in another cradle, part of which is seen at *g b b i*, supported by four wires from the point *f*.

In fig. 3. is seen a sphere of brass *d*, 6 inches in diameter, slung in a cradle *a b c*, by three wires* from the links *f*, suspended in a glass jar,† containing near four gallons of water, whose temperature is shewn by a thermometer at *e*.

* These wires were of such a size that 91 inches weighed 20,71 grains, consequently 1 inch = 0,2276 grain, and the three wires = 0,6828 grain; and their specific gravity being 8,7, their loss of weight, by sinking 1 inch in water, would be = 0,0785 grain. This correction it may be necessary hereafter to attend to.

† The glass jar is made somewhat conical, being in

	inches.	
Diameter at top - - -	12,0	
Ditto at bottom - - -	8,7	
Mean ditto - - - -	10,35	
Mean height within - - -	11,8	
Contents in cubic inches -	= 992,78	
Which is in ale gallons - -	= 3,8	= 15¼ quarts.

It may also be noted, that 1 inch in depth of the water near the top is = 113 cubic inches, which is equal to the exact bulk of the sphere, as will be seen hereafter.

(§. 11.) It was necessary to measure the exact size, and correctness of figure, of this sphere. For this purpose was made a wooden gauge or frame *abcde*, (Tab. V. fig. 2.) in which the sphere was placed, upon semicircular pieces within, lined with green cloth to prevent bruising it: upon this frame was placed a brass square *klmn*, whose sides were about $\frac{1}{100}$ inch in length more than the diameter of the sphere. This square, by raising or lowering the screws *o r s*, was easily made to coincide with a plane passing through the centre of the sphere. *p* is a micrometer screw, the interior extremity of which is brought just to touch the surface of the sphere, while the opposite side bears gently against the interior side of the frame at *o*; and, by turning the sphere round, so as to present different diameters to these points of contact, any variety in the diameter may be seen by the index *l*, and plate *q*, divided into 10,ooths inch. To render this operation more convenient, three great circles were drawn with a pencil upon the sphere, at 90° distance from each other, (the two former were traced by the artist in the lathe, while the sphere was making, and the third was drawn from them,) and each was divided into 8 equal parts. The immediate result of these experiments would only give the differences, and not the absolute quantity, of the diameter; for this purpose, a brass ruler *r*, fig. 3. was made, of such a length as just to go within the brass frame *klmn*; and, being substituted in the place of the sphere, could easily be compared with any given diameter, and afterwards measured with the divided scale, fig. 1. With these instruments I made the following observations, August 31, 1796, the thermometer being at 61°.

(§. 12.) *Examination of the Dimensions of the Brass Cube, by Means of the divided Scale.*

The microscope and micrometer being both adjusted, as well with respect to their focus,* as to the value of the micrometer scale, the cross wires in their focus were removed to a distance from each other of five inches *nearly* on the beam, (the former being at 27, and the latter at 32, inches,) and then *correctly* adjusted to this interval on the divided scale. I must observe, indeed, that the value of the micrometer scale was not exactly ten revolutions of the screw to $\frac{1}{10}$ inch, as Mr. TROUGHTON designed; but this measure by the screw,† from 6 trials, was deficient by — 0,0002 inch; *viz.* two ten-thousandths of an inch were to be added to each tenth of an inch measured by the micrometer, and so in proportion for a less quantity; but this correction is hardly worth notice.

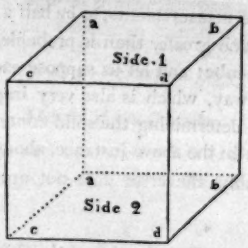
	On the scale. inch.	inch.	inches.
The interval of the cross wires in the microscope and micrometer -	27 and 32	=	5,0000
Interval of ditto, on another part of the scale, *viz.* - - - -	26 and 31	=	5,0000 —
Ditto, ditto - - - - -	25 and 30	=	5,0001 +

* The focal length of the object lens is - - - = Inch. 0,75
The distance of the cross wires from the object lens = 2,00
The focal length of the combined eye glass - = 1,50
Whence the magnifying power of the microscope becomes = 14,2 times.

† One Revolution of the screw of the micrometer was - = $\frac{1}{100}$ inch.
Each grand division, of which there were ten - = $\frac{1}{1000}$ inch.
These again subdivided into five, each became - = $\frac{1}{5000}$ inch.
And half a division, which is very visible, is - = $\frac{1}{10000}$ inch.

I therefore say, this interval was 5 inches correctly, to within less than the twenty thousandth part of an inch, on this scale.

Measurement of the Cube, viz. *of the Side* 1. (*See the Figure.*)



	Inches.		Inches.	Mean.
From *a* to *b* = 5 — ,0114		therefore =	4,9886	
a to *c* = 5 — ,0115	- -	=	4,9885	Inches.
c to *d* = 5 — ,0105	-	=	4,9895	= 4,98882
b to *d* = 5 — ,0113	- -	=	4,9887	

The Side 2.

From *a* to *b* = 5 — ,0106	- -	=	4,9894
a to *c* = 5 — ,0098	-	=	4,9902
c to *d* = 5 — ,0102	- -	=	4,9898
b to *d* = 5 — ,0112	-	=	4,9888

= 4,98955

Height of the Cube, from Side 1 *to Side* 2.

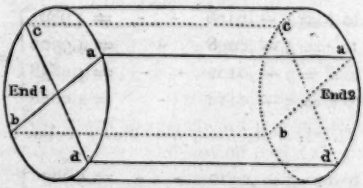
	Inches.		Inches.	
From *a* to *a* = 5 — ,0110	- -	=	4,9890	
b to *b* = 5 — ,0105	-	=	4,9895	
c to *c* = 5 — ,0107	- -	=	4,9893	= 4,98925*
d to *d* = 5 — ,0108	-	=	4,9892	

* It cannot escape notice, that all these measures were something less than 5 inches, the quantity proposed: it arose from this, Mr. Troughton informs me, he was more

(§. 13.) Now the three foregoing mean measures of the side of the cube, multiplied into each other, will give = 124,18917 cubic inches, for the contents of the brass cube; which must be very near the truth; for, if not, let us suppose the error, in taking each of these measurements, to be half a thousandth of an inch, which is much greater than is probable, *viz.* = $\frac{1}{10,000}$ part of the side of the cube; and let us suppose each of these errors to lie the same way, which is also very improbable; in that case, the error in determining the solid content would be only $\frac{3}{10,000}$ of the whole; in the above instance, about 0,03 cubic inch; but, more probably, the error does not amount to half this quantity.

(§. 14.) *Examination of the Cylinder.*

The micrometer and microscope of the divided scale (Tab. V. fig. 1.) being removed till their cross wires were four inches distant, *viz.* from 54 inches to 58 inches, and the thermometer at 62°, I observed, of the end or base of the cylinder, No. 1.



solicitous to obtain a true figure, than the exact size; neither of which however were very important, as both were to be proved by the mode I have adopted. What was important, was to have the sides true planes; and these were examined, as I am informed, by the reflected image of the moon, seen through a large telescope, the focus of which would be altered, if the surface were either hollow or convex.

	Inches.	Inches.	Mean. Inches.
The diameter $a\,b = 4 - ,0027 = 3,9973$			
$c\,d = 4 - ,0024 = 3,9976$			$= 3,99745$

End 2. of the Cylinder.

	Inches.	Inches.	
The diameter $a\,b = 4 - ,0014 = 3,9986$			
$c\,d = 4 - ,0029 = 3,9971$			$= 3,99785$

Height of the Cylinder.

The microscope and micrometer being placed respectively at 52,1 inches and 58,1 inches, *viz.* at the interval of exactly 6 inches on the scale, I found

	Inches.	Inches.	Mean. Inches.
The height from a to $a = 6 - ,0049$		$= 5,9951$	
b to $b = 6 - ,0047$		$= 5,9953$	
c to $c = 6 - ,0047$		$= 5,9953$	$= 5,99502$
d to $d = 6 - ,0054$			
Repeated - 58		$= 5,9944$	
56			

Now the mean diameter of the cylinder having been found

	Inches.
at the end 1	$= 3,99745$
at the end 2	$= 3,99785$
The factor for the square of the diameter of a circle, to find the area, being, as is well known,	$= 0,7854$
And the height of the cylinder	$= 5,9950$

The above four quantities, multiplied into each other, give for the contents of this cylinder, in inches, $= 74,94823$; and this result may be taken at least as correct as that of the cube, *viz.* to about the third place of decimals.

(§. 15.) Having adjusted the beam of the balance, (Tab. VI.) with respect to the length of its arms, its centre of gravity, and the three points of suspension of the beam and scales, and having examined the weights, I proceeded to the remaining parts of this experiment.

Sept. 2d, 1796. The balance beam adjusted by the screw *p*, till the vibrations were so slow as to require more than 50 seconds of time for each, $\frac{1}{100}$ grain appeared to move the index through three divisions* of the scale *s* and *t*, $= \frac{1}{7}$ inch, when the beam was not loaded; but, when the beam was loaded with 16384 grains, or near 3lb. Troy, $\frac{1}{100}$ grain was equal only to $0\frac{1}{2}$ division† of the same scale.

(§. 16.) Sept. 4th. The thermometer being at 63°, and the barometer at 29,86 inches.

	oz.	grains.	grains.
The weight of the counterpoise to the pan or scale for weighing the cube in air, was - - -	= 1	75,02	= 555,02

* 20 divisions are = 1,0 inch.

† That is, the beam was sensible with $\frac{1}{32000}$ part of the whole weight. Mr. HARRIS's beam, with which he and Mr. BIRD made their observations on the Exchequer weights, turned with $\frac{1}{32000}$ part of the whole weight, and was consequently only $\frac{1}{2}$ part so sensible as this. See "The Report of the Committee of the House of "Commons in 1758, to inquire into the original Standards of Weights and Measures "in this Kingdom, and to consider the Laws relating thereto." See also a second Report in 1759; both of which contain a vast deal of useful information on this subject, extending through fifty folio pages, and are to be found in the 2d volume of Reports, from 1737 to 1767. A bill was brought in, in consequence, but afterwards dropped; and it is much to be lamented, that this inquiry did not go to the full length of an act of parliament. Note farther, the largest of the beams, of which there are four of different sizes, now made use of in the dutchy court of Lancaster, for the actual sizing of the weights of the kingdom, is about 3 feet long, and is moveable with about 30 grains, when 56lb. avoirdupois are in each scale, *viz.* about $\frac{1}{11000}$ part of the whole.

To which, add the weight of the common pan with the silk lines, on the left arm of the beam, and marked with x, the common right-hand pan having been removed - - - -	- -	= 413,40 grains.
And the whole weight of the pan or apparatus for weighing the *cube in air*, becomes - - - -	- -	= 968,42
(§. 17.) The counterpoise to the pan or scale for weighing the cylinder in air, was found - - - -	= 1 oz. 72,34 grs.	= 552,34
To which, add the weight of the common pan on the left arm, as before	- -	= 413,40
And the whole weight of the pan or scale for weighing the *cylinder in air*, becomes - - - -	- -	= 965,74

Note, in the preceding and such like experiments, the common right-hand scale being removed, and the left-hand scale being always used, and always the same weight, *viz.* 413,40 grains, when either the cube, or cylinder, or any large body, is weighed, notice need only be taken of the counterpoise weights, *viz.* 555,02 grains, or 552,34 grains, respectively; and these are to be deducted from the general amount of all the weights in the left-hand scale, marked x; but it certainly would have been more convenient to have had single weights, ready adjusted, for these counterpoises, both in air and in water. These, though at first omitted, have since been supplied.

(§. 18.) The counterpoise to the scale for the cube, in distilled water, with the heat of 61° -	= 442,75 grains.
To this, add the weight of the common scale, as before	= 413,40

And we have the whole weight of the scale for the cube in water - - - - - -	= grains. 856,15
But the weight in air having already been found -	= 968,40
The difference of the weights - - -	= 112,25
Gives for the specific gravity of this brass -	= 8,62
(§. 19.) The counterpoise to the scale for the cylinder, in the same water, with the same heat -	= 441,68
To this, add the weight of the common scale, as before	= 413,40
And the whole weight of the scale for the cylinder, in water, becomes - - - - -	= 855,08
Its weight in air has already been found - -	= 965,74
The difference of these weights - - -	= 110,66
Gives for the specific gravity of this brass -	= 8,78
The mean specific gravity of this brass and brass-wire may therefore be put at about - -	= 8,7

N. B. The tables of specific gravity give that of wrought brass from 8,00 to 8,20. It was necessary to ascertain the specific gravity of the brass wire, to make the correction mentioned in the note to §. 10.; for, as it was highly probable, that in experiments with this hydrostatic balance, the scales for the cube and cylinder would occasionally be immersed to different depths in the water, and their weights would be altered, as more or less of the wires by which they were suspended remained out of the water;

I accordingly found, that 80 inches in length of this wire, used in the scales for the cube and cylinder, weighed in air - - - -	= grains. 6,16

And consequently, 1 inch would be = 0,077 grain, and four wires of 1 inch = ,308 grain; which, divided by the specific gravity, *viz.* $\frac{308}{8}$, would give 0,0354 grain, for the correction of

every inch that the scale was sunk lower in the water; and so in proportion.

(§. 20.) *Experiment of the Cube of Brass weighed in Air.*

The cube was suspended to the right arm of the beam, by the scale belonging to it, and the left scale pan, with the mark ×, was hung at the other end of the beam, in which were placed the following weights,* made by E. TROUGHTON.

		grains.
viz. No.	15 of	16384
	14 –	8192
	13 –	4096
	12 –	2048
	11 –	1024
	9 –	256
		84,82

The total weight of the cube in air = 32084,82 { the barom. being at Inch. 29,0 ; the therm. - at 62°,0

(§. 21.) *Experiment of the Weight of the Cylinder in Air.*

		grains.
No.	15 of	16384
	13 –	4096
	11 –	1024
		53,37
		21,557,37
But a counterpoise of	grains. 555,02	
having been used, by mistake, instead of -	552,34	= + 2,68
Add this excess =	2,68	

And the total weight of the cylinder is - - = 21,560,05 { the barom. at inch. 29,0 ; the therm. at 62°,0

* This scale contained also 555,02 grains, being the weight or counterpoise to the scale for the cube.

(§. 22.) *The Cube weighed in distilled Water.*

Sept. 5. Put into the left scale, the counterpoise { 300 / 100 } = 400,00 grains.
for the water scale - - -

The cube, with its scale, was then immersed in the water.

I then restored the equilibrium, by putting into the opposite or left-hand common scale, Mr. TROUGHTON'S weights, No. 10. - - - = 512,00 grains.
(The barom. standing at 29,47 inches, the therm. at 60°,2.) 200,
30,
3,70

But a counterpoise of - - 400 grains. having been taken, by mistake, instead of 442,75 } = 745,70 − 42,75
Deduct the difference, which was so left out = 42,75

The apparent weight of the cube in water becomes = 702,95
Add the correction * for the loss of weight of the 4 wires, by immersion 2¼ inches deeper than when the counterpoise was adjusted - } = + ,08
And the true corrected weight of the cube in water, with 60°,2 of heat, becomes - - } 703,03

* When the cube was immersed, the water in the glass jar stood 2¼ inches higher than when the counterpoise for this water-scale was adjusted, and found to be 442,75 grains; (see §. 18.) and 1 inch of alteration in the height of the water having appeared to be = 0,0354 grain in weight, (§. 19) 2¼ inches will be = 0,078 grain; and so much must be added, to correct for the loss of weight, in the four wires, that suspended the scale and cube in water. When the cube was immersed, the surface of the water stood 1,5 inch below the top of the glass jar, and 9,7 inch below the centre of the beam, or index point.

When the cube was in the water, the beam was clearly sensible with $\frac{1}{10}$ of a grain.

(§. 23.) *Experiment of the Cylinder in distilled Water.*

Sept. 5. The thermometer being at from 60°,2 to 60°,5, and the barometer 29,47 inches,

	grains.	
Put into the left scale pan, the counterpoise to the water-scale for the cylinder -	300 100 41,7	= 441,7

The cylinder, with its water-scale, was immersed in water. I then restored the equilibrium, by putting into the left scale,

	grains.
Mr. TROUGHTON's weights, No. 12 -	= 2048
No. 9 -	= 256
	200
	30
	10
	4
	1,10
Weight of the cylinder in water - -	= 2549,10
Add the correction for the loss of weight of the four wires, by being $1\frac{1}{2}$ inch deeper immersed in the water, than when the counterpoise was adjusted - -	= + 0,05*
Corrected weight of the cylinder in water -	= 2549,15

* In order that *this* and some other corrections may be the more easily applied, I have computed the 3 following tables, to be used whenever great accuracy is required.

After this experiment, I discovered that some small bubbles of air had insinuated themselves between the cylinder and the

Table I. Shewing the expansion of cast brass, both in length and solidity, and also of water, in solidity, by the effect of heat: the former is derived from Mr. SMEATON'S experiments; (Phil. Trans. Vol. XLVIII.) and the latter from some of my own, when I was a resident member of the University of Oxford.

Degrees of Heat.	Expansion of Brass.		Expansion of Water.
	In Length.	In solidity.	In solidity.
°	Millionth Parts.	Millionth Parts.	Millionth Parts.
1	1	3	165
2	2	6	330
3	3	9	495
4	4	12	660
5	5,2	16	825
6	6	19	990
7	7	22	1155
8	8	25	1320
9	9	28	1485
10	10,4	31	1650

Table II. Shewing the Correction for the Wires, or the Diminution of the Weight of the Water-Scales, by Immersion in Water.

By Immersion in Water.	The 4 Wires of the Cube, or Cylinder.	The 3 Wires of the Sphere lose
Inches.	Grains.	Grains.
1	— 0,035	— 0,078
2	— 0,071	— 0,157
3	— 0,106	— 0,235
4	— 0,142	— 0,314
5	— 0,177	— 0,392
6	— 0,212	— 0,471
7	— 0,248	— 0,549
8	— 0,283	— 0,628
9	— 0,319	— 0,706
10	— 0,354	— 0,785
20	— 0,708	— 1,570

N. B. 80 inches in length, of the wires for the scales for the cube and cylinder weigh - - - } 6,16 grs.

therefore 1 inch will be ,077 grain, and 4 wires of 1 inch - } = 0,308

Also, 91 inches of the wire for the sphere weigh - - - } = 20,71

and 1 inch = 0,227, and 3 wires of 1 inch - - - } = 0,683

and the specific gravity of the wire is = 8,7

Table III. Shewing the Correction of the Weight of the Sphere in Air, on Account of the Weight, or Heat, of the Atmosphere.

Barometer	Correction.	Therm.	Correction.
Inch. 1/0.	Grains.	°	Grains.
29,5	0,00	50	0,00
1	— ,12	1	+ 0,10
2	,23	2	0,20
3	,35	3	0,30
4	,47	4	0,40
5	,58	5	0,50
6	,70	6	0,60
7	,82	7	0,70
8	,94	8	0,80
9	1,05	9	0,90
10	1,17	10	1,00

N. B. If the barometer is below 29½ inches, or the thermometer below 50°, use the contrary signs.

Water being taken as heavier than air, as 836 : 1, (see Observations in Savoy, Phil. Trans. for 1777,) the barometer being at 29,27, and thermometer 51°, a sphere of air equal in bulk to the brass sphere, viz. = 113¾ cubic inches, would weigh, when the barom. was 29,5 inch. and the therm. 50° = 34,57 grains; and 1 cubic inch of such air - = 0,304

This correction will serve for any other body whose bulk is known.

scale in which it hung; these therefore were removed, and the experiment repeated, as follows:

		grains.
Weights as before	No. 12 =	2048
	No. 9 =	256
		200
		30
		10
		4
		1,10
The buoyancy of the air bubbles being removed = +		8
		1,07
Add the correction for the loss of weight in the wires, as before = +		2553,17
		0,05
And the more exact weight of the cylinder in water becomes =		2553,22

with the temperature 60°,5
and the barometer 29,47 inches.

Note, when the cube was weighed in water, its centre was below the surface of the water = 2,5 inches.
the cylinder was weighed in water, its centre was below the surface of the water = 3,7 inches.

that is, the cylinder was the deepest by - - = 1,2

The repetition of this experiment shews how necessary it is to attend to the most trifling circumstances: there were not more than three or four of these particles of air, and those not larger than a small pin's head. Moreover, it may be noted, the distilled water in which these experiments were made, being afterwards examined with my (MARTIN's) hydrometer, in the heat of 60°¼, weighed on that scale = 1,0005; so that I see no reason for diffidence in the quality of the water.

(§. 24.) *A Synopsis of the preceding Experiments.*

	Cube.	Therm.	Cylinder.	Therm.	Barom.
	Inches.	°		°	Inches.
Contents (true to $\frac{1}{10000}$) in inches	124.18917	61	74,94826	62	
	grains.				
Weight in air, true to 0,02 grain	32084.82	62	21560,05	62	29,00
Weight in water, true to 0,10 grain	705,03	60,2	2553,22	60,5	29,47
Weight of an equal bulk of water, true to 0,12 grain, or $\frac{1}{10000}$	31381,79		19006,83		
Weight of a cubic inch of water, from these experiments -	252,694		253,600 *		

The diversity in the result of these two experiments is deserving of notice, and must be explained. It may proceed from two causes, which we will now inquire into. But first it may be observed, that the accuracy in measuring the dimensions of these two bodies, as well as the precision in weighing them, has, I think, been such as to put out of all doubt this part of the experiment. From whence then does this difference arise? Either of two causes may be suspected; *viz.* the pressure of the water against the sides of these two bodies altering their volumes, which, it may be presumed, would have a greater effect on the cube, from its figure, than on the cylinder, and in a direction agreeable to this difference; that is, it would diminish the capacity of the cube more than that of the cylinder, and thus make the apparent weight of a cubic inch less in the experiment of the cube. But also we see, that the cylinder was

* The weight of a cubic inch of common or rain water has been reckoned about 253 grains, sometimes = 253,33 grains, at others 253,18. But authors do not seem to have agreed in what they meant by *common* water, *rain* water, *pump* water, *spring* water, and *distilled* water; for occasionally they are all confounded, and made to pass for each other; and sufficient notice seems not to have been taken of the temperature to which these weights were assigned. See MARTIN'S *Philosophia Britannica*. LEWIS'S Philosophical Commerce of Arts. CHAMBERS'S Dictionary, by Dr. REES. *&c. &c.*

weighed at a greater depth, by 1,9 inch, than the cube, below the surface of the water. Now, if it be true that water is compressible,* it will become denser, from its weight, at different depths, and this circumstance would act in the same way with that just mentioned; *viz.* would make the apparent weight of a cubic inch less from the experiment of the cube than the cylinder, which we see is the fact.

(§. 25.) In order to dissipate these doubts, I caused a very accurate hollow brass sphere to be made, of about six inches diameter, and of such thickness of metal, *viz.* 0,13 inch, as to be very little heavier than water, and yet of such strength as, together with its form, to resist any probable change of bulk by the pressure of water.

This sphere, which has already been mentioned, (§. 10.) was examined in the following manner. The six-inch moveable bar *r*, (Tab. V. fig. 3.) of the gauge, was compared with the divided scale of inches, fig. 1. The microscopes being adjusted to exactly six inches, or the interval between 26 inches and 32 inches, and the bar placed under them, the excess above 6 inches was found to be as follows, by the micrometer, *n o*.

1st trial. inches.		2d. trial, after re-adjustment. inches.	
6+,0055		6+,0055	
,0053	thermom.	,0052	therm.
,0056	64°,0	,0055	64°,0
,0054		,0054	
,0057		,0052	
Mean of the 1st trial = 6 ,00550		6 ,00536	

Mean of the 2d trial = 6 ,00536

Mean of both, or length of bar = 6 ,00543 in the temperature of 64°.

* See Mr. Canton's Experiment in the Phil. Trans. Vol. LII.

(§. 26.) The bar was then placed in the rectangular *gauge k l m n*, fig. 2. in the direction *p o*; and the end of the micrometer screw brought to bear against it repeatedly, so as to touch without force, or considerable pressure; and the divisions* cut by the index, on the micrometer plate of the gauge, were as follow:

Trial 1st. Division on the micrometer.		Trial 2d. Division on the micrometer.		Trial 3d. Division on the micrometer.	
65	therm.	64	therm.	$64\frac{1}{2}$	thermom.
63		62		65	
66		65		66	
70	62°,0	68	62°,0	63	62°,8
66		$62\frac{1}{2}$		$62\frac{1}{2}$	
Mean = 66		63,3 †		64,2	

The mean of these three means is 64,5, with the temperature 62°,1.

(§. 27.) The bar was now removed from the gauge, and the sphere put there in its place; and, by means of the three great circles, each of which was divided into 8 equal parts, nine several diameters of the sphere were taken, as follow:

	Div. of microm.			Div. of microm.			Div. of microm.			Div. of microm.	
diam. A B	40	therm.	diam. G H	40	therm.	diam. C D	41	therm.	diam. I K	$44\frac{1}{2}$	therm.
	50			42			49			46	
	47			45			48			47	
	42	62°$\frac{1}{2}$		42	62°,5		42	62°,4		45	62°,5
	46			44			$44\frac{1}{2}$			46	
Mean=45				42,6			43,9			45,6	

* Each thread of this screw is $= \frac{1}{50}$ inch, and each revolution of the screw is divided into 100; so that every division on the micrometer plate is $= \frac{1}{5000}$ inch.

† In all these experiments with the gauge, the figures on the micrometer plate increase as the screw goes *forward*; *viz.* the higher numbers indicate a less interval or diameter.

The above four mean dimensions may be called *equatorial, viz.* - - - - - - - $\left\{\begin{matrix}45\\42,6\\43,9\\45,6\end{matrix}\right.$

The mean of which is - - - - = 44,8

Div. of micr.		Div. of micr.		Div. of micr.	
diam. E F	44, 46, 44, 45, 45 therm. 62°,5	diam. 1,2.	42, 45, 45, 40, 41 therm. 62°,6	diam. 3,4.	40, 44, 41, 40, 41 therm. 62°,8
Mean	44,8		42,6		41,1

These three last dimensions, together with the 1st of the preceding set, may be called *meridional*, being in a circle at right angles to the former, *viz.* - - - - - - $\left\{\begin{matrix}A\,B = 45\\E\,F = 44,8\\1,2 = 42,6\\3,4 = 41,1\end{matrix}\right.$

The mean of which is - - - - - = 43,4

and differs from the former not quite $\frac{1}{10000}$ inch.

In another great circle, 90° from the preceding, comprising the diameters already taken, E F and C D, at the intersection of the two former circles, were taken

Div. of micr.		Div. of micr.	
The diameter $\alpha\,\beta$	41, 44, 44, 41, 40 therm. 63°	diameter $\gamma\,\delta$	40, 41, 42, 40, 42 therm. 63°,1

The diameters

E F, C D taken as before $\left\{\begin{matrix}44,8\\43,9\end{matrix}\right.$

$\alpha\,\beta$ - - - 42,5

$\gamma\,\delta$ - - - - 41,0

Mean - - = 43,0, which is that of another great circle

or meridian, at right angles to the former; from whence it will be seen, that not one of the three circles differs from another more than about $\frac{1}{10000}$ inch.

The preceding 9 mean dimensions of the diameter, collected, are

A B = 45,		
C D = 43,9		
G H = 42,6		
I K = 45,6	the mean of which is	= 43,7
E F = 44,8	in the temperature -	62°,6
1,2 = 42,6		
3,4 = 41,1		
$\alpha\beta$ = 42,5		
$\gamma\delta$ = 41,0		

Now the import of the foregoing experiments is this, that when the mean diameter of the sphere is holden between the points of contact of the gauge, near *o* and *p*, the index of the micrometer shews - - - = 43,7 divis.
but, when the bar *r* is placed there, it shews = 64,5
the difference is - - - = 20,8
and by so much is the bar shorter than the diameter of the sphere.

These divisions, 20,8, are equal to (§. 26.) - inches. 0,00202
and the length of the bar has already (§. 25.) been found = 6,00543
therefore the true diameter of the sphere becomes = 6,00745
which quantity I think must be true to within $\frac{1}{10000}$ inch.

(§. 28.) The cube of this diameter, 6,00745 inches × ,5236, as is well known, will give the contents of the sphere in cubic inches, *viz.* = 113,5194 inches, which must be very near the truth: for, if not, let it be supposed that the inaccuracy in the measurement, or the irregularities in the figure of this sphere,

should be such as to amount to $\frac{1}{1000}$ inch, and these so many, without balancing each other, as to produce a spheroidical form, one of whose diameters should exceed the other by $\frac{1}{1000}$ inch; in that case, the error in the assumed solid would not exceed $\frac{1}{3000}$ part of the whole; and this is a position infinitely too extravagant to be admitted, when we recollect, that this diameter has been probably taken to within a tenth part of that error.

(§. 29.) The weight of this sphere, in air and in water, comes next under our consideration; the experiments for which were as follow, made June 12, 1797; the barometer being at 29,74 inches, and the thermometer, in air, at 67°.

Experiment the 1st.

The weight of the sphere in air, the counterpoise, or weight of the scale or cradle, *a b c f*, (Tab. VII. fig. 3.) in which the sphere hung, being allowed for *, so that *this* was the net weight -	=	Troy grains. 28722,64
The sphere and scale suspended in water, with its centre 5,6 inches below the surface, and the heat 66°	=	grains. 303,17
Deduct the counterpoise, or weight of the scale, in water, with the same heat of 66°, and same depth† below the surface -	=	— 253,32
The difference is the net weight of the sphere in water, of the temperature 66°, which, deducted from its weight in air - -	—	49,85
Leaves the weight of a bulk of water = the sphere, in the temperature 66°, and 5,6 inches below the surface - - - - -	=	28672,79

* The weight of this scale, with its 3 wires, in air, was = 276,10 grains.

† The sphere having been weighed in the same depth of water that the counterpoise to the scale was determined in, no correction for the greater or less immersion of the scale-wires was here necessary; which however will sometimes be the case. See §. 29. and table II. of correction, §. 23.

Experiment the 2d. June 16, 1797.

The barometer being at 30,13 inches, and the thermom. at 68°.

		grains.
Weight of the sphere, together with the scale, in air		29265,91
Deduct the weight of the scale, or counterpoise, in air	= −	544,03
Remains the total net weight of the sphere in air	=	28721,88
And, to reduce this to the same state of the atmosphere as the preceding observation, *viz.* 29,74 inches of the barometer, add the correction for 0,39 inch (see table, §. 23.)	= +	,46
Also the correction for 1° of the thermometer	= +	,08
And the net weight of the sphere, in an atmosphere of 29,74 inches, and heat of 67°, becomes	=	28722,42

		grains.
Weight of the sphere, with its scale, in water, 3,7 inches below the surface, and the thermometer at 66°,1	=	484,70
From thence deduct the weight of the scale in water	=	435,09
The net weight of the scale in water becomes	=	49,61
To which, add the correction for the wires of the scale being immersed 2,53 inches deeper now, than when its weight in water was determined (see table, §. 23.)	=	0,20
And the corrected net weight, in water, is		49,81
Which, deducted from its weight in air, leaves the weight of a bulk of water = the sphere, in temperature 66°,1	=	28672,61
Correction for 0°,1 of heat*	+	,45
And the true corrected weight of a bulk of water equal to the sphere, reduced to the barometer = 29,74, and therm. 66°,0, becomes	=	28673,06

* One degree difference of heat in the water will alter the weight of the sphere in water, or the weight of the bulk of water equal to it, = 4,54 grains; so that, by far the greatest source of error, in these experiments, lies in the difficulty of exactly knowing, and preserving, the temperature of the water.

Experiment the 3d. June 16, 1797.

The true net weight of the sphere in air, reduced to a state of the barometer of 29,74 inches, and therm. 67°, as in last experiment	=	grains. 28722,42
Weight of the sphere, together with its scale, in water, 6,8 inches below the surface; the thermom. at 66°,1 = grains. 484,20		
Deduct the weight of the scale in water 435,09		
The difference is the net weight of the sphere in water, of the temperature 66°,4 - - = 49,11		
To which, add the correction for the wires of the scale being immersed 5,5 inches deeper now, than when its weight in water was determined (see table, §. 23.) - + ,44		
The corrected net weight, in water, becomes	=	49,55
Which, deducted from its net weight in air, leaves the weight of a bulk of water = the sphere, and 6 inches below the surface, with the heat of 66°,4 - - - -	=	28672,87
Correction for 0°,4 of heat (see table, §. 23.)	= +	1,81
The true corrected weight of a bulk of water = the sphere, in the heat of 66°,0, and with a pressure of the barometer of 29,74 inches, and 6 inches below the surface -	=	28674,68

(§. 30.) *Results of the Observations of the Sphere collected.*

Correct weight of a bulk of water = sphere, the barom. being at 29,74 inches, therm. 66°,0.	grains.	At a depth below the surface of the water. inches.
By the 1st observation - - -	28672,79	5,6
2d observation - - -	28673,06	3,7
3d observation - - -	28674,68	6,8
Mean of all - - - - -	28673,51	5,37

Which, I think, may fairly be presumed to be within 1 part in 50,000 of the truth.

(§. 31.) Now the contents of this sphere having already (§. 28.) been found to be = 113,519 cubic inches; $\frac{28679.51}{113.519}$ = 252,587 grains, will be the weight of a cubic inch of distilled water, under the circumstances above mentioned, by Mr. TROUGHTON'S weights.*

I think it may now be concluded, that the variety in the experiments of the cylinder and the cube, (§. 24.) does not proceed from the different depths† in the water, at which they were made; at least, that the pressure of 9 inches, in perpendicular height of water, does not render that fluid more dense by $\frac{1}{50,000}$ part, which may be reckoned an insensible quantity; but that this variety *did* proceed from a difference in the yielding of the sides of the cube and the cylinder. And lastly, I hope it may be trusted, that the weight of a bulk of water

* But, as will appear hereafter, (§. 41.) these weights are too light, when compared with the standard in the House of Commons, by about 1 in 1523,92; the correction therefore, for this difference, would be = 0,165 grain, to be deducted

from - - - - -	252,587 grains.
	— ,165
And the weight of a cubic inch of distilled water, in grains of the parliamentary standard, will be - - -	= 252,422

† By means of an alteration and addition to my apparatus, since the experiment abovementioned was made, I have been able to repeat it at greater depths below the surface of the water, *viz.* when the centre of the sphere was 5 inches, 13 inches, and 21 inches, below, without any appearance of water having a sensible difference of density at different depths. The vessel I used for this purpose was of wood, 32 inches high, and 12 square, containing 16 gallons, with two sides of plate-glass, to admit the light; and the wires by which the sphere was suspended were 45 inches long, and stronger than before, *viz.* 100 inches of the single wire weighed 24,14 grains; and due allowance was made for the different weight of the scale and wires, in air and water, from actual experiment.

$\frac{1}{3}$ the sphere, has been determined to within $\frac{1}{20,000}$ of the whole, and probably to within half that quantity.

(§. 32.) Having then, through the means of Mr. WHITEHURST's observations, and of his own instrument, ascertained the length of his proposed standard, in the latitude of London, 113 feet above the level of the sea*, under a density of the atmosphere corresponding to 30 inches of the barometer, and 60° of the thermometer, which is full as satisfactory, for all practical purposes, as if it had been done *in vacuo*†, which I conceive to be nearly impossible; and, having determined the weight of any given bulk of water, compared with this common measure; I believe it now only remains, to ascertain the proportion of this common measure and weight, to the commonly received measures and weights of this kingdom.

(§. 33.) It is perfectly true, that if I chose to indulge in fanciful speculation, I might neglect these comparisons, as an unphilosophical condescension to modern convenience, or to ancient practice, and might adopt some more magnificent integer than the *English pound* or *fathom;* such as the *diameter* or *circumference* of the world, *&c. &c.* and, without much skill in the learned languages, and with little difficulty, I might ape the barbarisms of the present day. But in truth, with much inconvenience, I see no possible good in changing the quantities, the divisions, or the names of things of such constant recurrence in common life; I should therefore humbly submit it to the good sense of the people of *these* kingdoms at least, to

* The height, as I have been informed, of the room of Mr. WHITEHURST's observations.

† It is perfectly true, that this supposes the experiment to be made with a pendulum similar to Mr. WHITEHURST's.

preserve, with the measures, the language of their forefathers. I would call a yard a yard, and a pound a pound, without any other alteration than what the precision of our own artists may obtain for us, or what the lapse of ages, or the teeth of time, may have required.

(§. 34.) The difference of the length of the two pendulums, from Mr. WHITEHURST'S observations, appearing to be 59,89358 inches, on Mr. TROUGHTON'S scale; and a cubic inch of distilled water, in a known state of the atmosphere, having been found to weigh 252,587 Troy grains, according to the weights of the same artist, it remains only to determine the proportions of these weights and measures, to those that have been usually, or may be fitly, considered as the standards of this kingdom; and herein a small discrepancy between themselves, in these authoritative standards, will have no influence on the general conclusion I propose to draw; which is, not so much to say what *bas* been the standard of Great Britain, as what it *sball* be henceforward, and may be immutably *so*; and which shall differ but a very small quantity, and that an assignable one, from those that have been in use for two or three hundred years past. By these means, no inconvenience would be produced from change of terms, or subdivisions of parts, or from sensible deviation from ancient practice: all that will be done, will be to render that certain and permanent, which has hitherto been fluctuating, or liable to fluctuation. To give effect and energy to these suggestions, is the province of another power.

(§. 35.) The chief standards of longitudinal measure, as far as I can learn, that carry any authority with them, are those preserved in the Exchequer; in the House of Commons; at the

Royal Society; and in the Tower. The first alone, indeed, bear legal authority, and have been in use for more than 200 years; the last is considered as a copy of them, and is not used for sizing generally. The two remaining ones are of modern date; and, although they do not carry with them *at present* any statuteable authority, yet, from the high reputation and acknowledged care of the artists who made them, (the celebrated Mr. GEORGE GRAHAM, and Mr. JOHN BIRD,) are undoubtedly entitled to very great respect; and are probably derived from a mean result of the comparisons of the old and discordant ones in the Exchequer. I shall begin with that of Mr. GRAHAM, which contains also the length of the Tower standard laid down upon it; will proceed then to Mr. BIRD's, and finally conclude with those at the Exchequer.

(§. 36.) May 5, 1797. I went to the apartments of the Royal Society, at Somerset House, and, with the ready assistance of Mr. GILPIN, at the kind instance of Sir JOSEPH BANKS, I made the following observations on Mr. GRAHAM's* brass standard yard, made in 1742. This scale is about 42 inches long, and half an inch wide, containing three parallel lines engraven thereon, on the exterior and ulterior of which are three divisions, expressing feet; with the letter E at the last division, and, by a memorandum preserved with it in the archives of the Society, is said to signify English measure, as taken from the standard in the Tower of London. That with the letter F denoting the length of the half of the French toise; put on here, by the authority and under the inspection of the Royal Academy of Sciences, then

* This rod was not made by Mr. GRAHAM, but, at his instance, procured by him from Mr. JONATHAN SISSON, a celebrated artist of that time. See Phil. Trans. Vol. XLII.

subsisting at Paris, to whom it was sent in 1742, for the purpose of comparing the French and English measures. The middle line, marked EXCH. of the three abovementioned, denotes, as is supposed, the standard yard from the Exchequer.

(§. 37.) This bar of Mr. GRAHAM's had been previously laid together with my scale divided by Mr. TROUGHTON, for twenty-four hours, to acquire the same temperature; they were also of the same metal, and, by placing it under my microscopes, adjusted to the interval between 10 and 46 inches, I found the interval on the Tower standard exceed

	Inches.	
mine, by —	0,00127	
	,00135	= the total length therefore 36,00130
	,00128	inches, the thermometer at 60°,8.
Mean =	,00130	

The interval on the line marked EXCH. was shorter than

	Inches.	
mine by -	— ,0066	
	,0066	= the total length = 35,9933
	,0068	inches, the therm. at 60°,6.
	,0067	

And the Paris half-toise, which had been supposed by the Academy to be = 38,355 English inches, was found, compared

	Inches.	
with mine, to be =	38,3561	
	,3563	Mean = 38,3561 *.
	,3559	

* Dr. MASKELYNE says, this standard yard of Mr. GRAHAM's was $\frac{1}{1000}$ inch longer on three feet than Mr. BIRD's divided scale, which he generally made use of in all his operations of dividing; and, from one made conformably to this of Mr. BIRD's, Mr. TROUGHTON divided my scale of 60 inches. This remark seems to

	Inches.
The 1st of the preceding observations giving - -	36,0013
The 2d - - - -	35,9933
The mean length of Mr. GRAHAM's standard becomes	35,9973.

agree with my 1st and 3d comparison, but not with the intermediate one. See Phil. Trans. for 1768, p. 324.

As I am now upon the subject of foreign measure, it may not be quite out of place to say a word on the length of the ancient Roman foot, which I am enabled to do with some precision.

Some years ago, when I was in Italy, I had several opportunities of ascertaining the length of this measure, by actual examination of the Roman foot rules, of which I have met with nine, *viz.* two in the Capitol at Rome; one in the Vatican; five in the Museum at Portici, near Naples; and lastly, one in the British Museum, sent from Naples by Sir WILLIAM HAMILTON. They were all of brass, except one half-foot, of ivory, with a joint in the middle, resembling our common box or ivory rules: and, by reference to my journal kept at that time, I find the mean result from all the nine rules, *viz.* by taking both the whole and the parts of each, (for they were divided into 12 inches, and also into 16ths, or digits,) gave, for the length of the old Roman foot, in English inches, correspondent to Mr. BIRD's measure, = 11,6063.

In confirmation also of this conclusion, and agreeably to the idea of Mons. DE LA CONDAMINE, in the "Journal of his Tour to Italy", I took the dimensions of several ancient buildings, *viz.* the interior diameter of the temple of Vesta; the width of the arch of Severus; the door of the Pantheon; and the width of the base of the quadrilateral pyramid of Cestius, which, it is curious to observe, I found exactly 100 old Roman feet, and 125 feet high. This I do not remember to have seen noticed by any former traveller.

The mean result of these experiments gave me -	11,617 English inches.
Ditto, as before, from the rules - - -	11,606 ditto.
The mean of the two modes of determination is -	11,612 ditto.

I may add, that in the Capitol is a stone, of no very ancient date however, let into the wall, on which is engraven the length of several measures, from whence I took the following:

The ancient Roman foot, = 11,635	English inches.
The modern Roman palm, = 8,82	ditto.
The ancient Greek foot, = 12,09	ditto.

(§. 38.) From the information in the report of a committee of the House of Commons, that sat in the year 1758, I learnt that Mr. BIRD'S parliamentary standard had been in the custody of some of its officers, but of whom nobody knew: however, under the authority of the speaker, who was so good as to furnish me with a room in his house, to make the comparisons in, I at last discovered this valuable original in the very safe keeping of ARTHUR BENSON, Esq. Clerk of the Journals and Papers, and which, I believe, had never seen the light for five-and-thirty years before. It is a brass rod or bar, about 39 inches long, and 1 inch square, inclosed in a mahogany frame, inscribed "Standard $\underset{\text{Geo. 2d.}}{\text{♔}}$ 1758"; at each extremity of it is a gold pin, of about $\frac{1}{10}$ inch in diameter, with a central point, and these points are distant = 36 inches. It bears, however, no divisions; but there was found with it, in another box, a scale divided into 36 inches, with brass cocks at the extremities, for the purpose of sizing or gauging other scales or rules by. Besides these, I found another standard, in size, and in all respects, similar to the last, inscribed 1760, having been made for another committee, that sat in that year; this also was accompanied with a similar divided scale of 36 inches.

These bars being too thick to be conveniently placed under the microscopes of my instrument, the interval of 36 standard inches was laid down on my scale with a beam-compass, two fine points made, and, compared with TROUGHTON'S divisions, was = 36,00023 inches; the thermometer being at 64°. I then examined the other standard, marked "Standard, 1760",

and found it to agree exactly with that of 1758; at least it did not differ from it more than ,0002 inch*.

(§. 39.) I was now to examine the old standards kept in the Exchequer: these Mr. CHARLES ELLIS, Deputy Chamberlain of the Tally Court at the receipt of the Exchequer, was so good as to supply me with; *viz.* the standard yard of the 30th of Eliz. 1588, and also the standard ell of the same date. These are what have been constantly used, and are indeed the only ones now in use, for sizing measures of length†. They are made of brass, about 0,6 inch square, and are very rudely divided indeed, into halves, quarters, eighths, and sixteenths; the lines being two or three hundredths of an inch broad, and not all of them drawn square, or at right angles to the sides of the bar, so that no accuracy could possibly be expected from such measures. However, the middle point of these transverse lines, between the sides of the bar, was taken as the intended original division; and these divisions, such as they were, were transferred, by a dividing knife, to the reverse side of my brass scale made by Mr. TROUGHTON, the thermometer being at 63°; and, at my leisure afterwards, I found as follows.

The ends of these venerable standards having been bruised a little, or rounded, in the course of so many years' usage, I conceived a tangent to be drawn to the most prominent part, which was about the centre or axis of the bar, and this point

* These quantities then being so small, I shall consider them as wholly insensible; and shall say, that Mr. BIRD's parliamentary standards of 3 feet exactly correspond with Mr. TROUGHTON's scale.

† There was also a standard yard of Henry VII. but of very rude workmanship indeed; now quite laid by, and at what time last used, no information remains: but of this more hereafter.

being referred to TROUGHTON'S scale, between 6 and 42 inches, the entire yard of 1588, measuring from one extremity to the other, was found to be shorter than this, by —,007 inch: but these comparisons will be better exhibited in a table.

Exchequer standard of 1588.	Difference from Troughton.	Length in Inches.	Difference on 36 inches.	Mean difference on 36 inches.
	Inch.			
Entire yard - -	— ,007	35,993	— ,007	
$\frac{1}{2}$ yard, from 24 to 42 inch.	+ ,063	18,063	+ ,126	Inch.
$\frac{1}{4}$ yard, from 15 to 42 inch.	— ,008	26,992	— ,011	= + 0,015
$\frac{7}{8}$ yard, from 10$\frac{1}{2}$ to 42 in.	+ ,022	31,522	+ ,025	
$\frac{1}{12}$ yard, from 8$\frac{1}{4}$ to 42 in.	— ,055	33,695	— ,059	
Entire ell, from 2 to 47 inch. - - -	— ,036	44,964	— ,029	
$\frac{1}{2}$ ell, from 2 to 24$\frac{1}{2}$ inch.	+ ,032	22,532	+ ,052	
$\frac{1}{4}$, from 2 to 35$\frac{1}{4}$ inch.	+ ,017	33,767	+ ,018	= + 0,016
$\frac{7}{8}$, from 2 to 41,375 inch.	— ,001	39,374	— ,001	*Viz.* the Exchequer measure is by so much the longer, or about 1 in 2322.
$\frac{1}{12}$, from 2 to 44,1875 inch. - -	+ ,051	42,239	+ ,043	

(§. 40.) It appears then, from the above table, that the ancient standards of the realm differ very little from those that have been made by Mr. BIRD, or Mr. TROUGHTON, and consequently, even in a finance view, (if one might look so far forward,) nothing need be apprehended, of loss in the customs, or excise duties, by the adoption of the latter.

(§. 41.) I shall now endeavour to shew the proportion of the *weights* that I have used, compared with the standards that were made by Mr. HARRIS, Assay Master of the Mint, under the orders* of the House of Commons, in the year 1758. They are kept in the same custody with Mr. BIRD's scales of length, and appear to have been made with great care, as a mean result from a great number of comparisons of the old weights in the Exchequer, which have been detailed at length in that report. Mr. HARRIS having been of opinion that the Troy pound was the best integer to adopt, as the standard of weight, I venture to conclude that *this* was the most accurate, and most to be depended upon, of all the various weights and duplicates that he made for the use of this committee; for he made them of 1, 2, 4, 8, 16, lb. and of $\frac{1}{2}$, 1, 2, 3, 6 ounces. It will therefore be sufficient for my purpose, to compare the 1 and 2 pounds Troy, and their duplicates, with the weights of Mr. TROUGHTON.

I did this, June 2d, 1797; the barometer being at 29,72 inches, and thermometer 67°.

	TROUGHTON's weights.		
	lb.	grains.	grains.
The standard weight of 1 Troy pound, or 5760 grains, marked 1758, kept at the House of Commons, in a small box by itself, by Mr. BENSON, weighed -	= 1	3,75 ,74	= 5763,745
A duplicate of the preceding, kept with some other weights, in a box marked B	= 1	3,70 ,67	= 5763,685
The mean weight of the Troy pound, from these two - - - - - -			= 5763,715

* See the report referred to in the note of page 148.

	Troughton's Weights, grains.	grains.
The two-pounds weight, from the House of Commons, kept in a deal box, marked A -	10000 1000 400 100 20 7 0,84	= 11527,84
A duplicate of the last mentioned 2 lb. weight, preserved in a deal box, marked B - - - -	10000 1000 400 100 20 7 0,55	= 11527,55
The thermometer *now* stood at 68°.		
Therefore the mean weight of 2 lb. Troy, from the two last trials, is - - -		= 11527,70
And consequently 1 lb. becomes - - -		5763,85
But, from the examination of the two single pound weights, as above, 1 pound is - -		5763,71
Therefore the mean of all is - - -		= 5763,78

That is, Mr. TROUGHTON's weights are too light by $\frac{b^2}{5760,00} = 0,6562$ grain on 1000 grains, or 1 in 1523,92 grains.

(§. 42.) In conclusion, it appears then that the difference of the length of two pendulums, such as Mr. WHITEHURST used, vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the sea, in the temperature of 60°, and the barometer at 30 inches, is = 59,89358 inches of the parliamentary standard; from whence all the measures of superficies and capacity are deducible.

That, agreeably to the same scale of inches, a cubic inch of pure distilled water, when the barometer is 29,74 inches, and

thermometer at 66°, weighs 252,422 parliamentary grains; from whence all the other weights may be derived.

As a summary of what has been done, I hope it may now be said, that we have attained these three objects;

1st. An invariable, and at all times communicable, measure of Mr. BIRD's scale of length, now preserved in the House of Commons; which is the same, or agrees within an insensible quantity, with the ancient standards of the realm.

2dly. A standard weight of the same character, with reference to Mr. HARRIS's Troy pound.

3dly. Besides the quality of their being invariable, (without detection,) and at all times communicable, these standards will have the additional property of introducing the least possible deviation from ancient practice, or inconvenience in modern use.

(§. 43.) Before I close this Paper, after having said so much on the subject of weights and measures, it may not be improper to add a few words upon a topic that, although not immediately connected, has some affinity to it; I mean the subject of the prices of provisions, and of the necessaries of life, *&c.* at different periods of our history, and, in consequence, the depreciation* of money. Several authors have touched incidentally upon this question, and some few have written professedly upon it; but they do not appear to me to have drawn a distinct conclusion from their own documents. It would carry me infinitely too wide, to give a detail of all the facts I have collected; I shall therefore content myself with a general table of their

* The various changes that have taken place, by authority, in different reigns, either in the weight or alloy of our coins, are allowed for in the subsequent table.

results, deduced from taking a mean rate of the price of each article, at the particular periods, and afterwards combining these means, to obtain a general mean for the depreciation at that period; and lastly, by interpolation, reducing the whole into more regular periods, from the Conquest to the present time: and, however I may appear to descend below the dignity of philosophy, in such œconomical researches, I trust I shall find favour with the historian, at least, and the antiquary.

A Table exhibiting the Prices of various Necessaries of Life, together with that of from the Conquest to the present Time, derived from respectable Authorities; wi added, the Mean Appreciation of Money, according to a Series of Intervals of 50 Periods, deduced by Interpolation.

Year of our Lord	Wheat, per Bushel.	THE PRICES OF VARIOUS ARTICLES / Miscellaneous Articles. Cattle in Husbandry. Horse.	Ox.	Cow.	Sheep.	Hog.	Poultry. Goose.	Hen.	Cock.
	s. d.	£. s. d.	£. s. d.	£. s. d	£. s. d.	£. s. d.	s. d.	s. d	s. d.
1050	0 $2\frac{1}{4}$	1 17 6 • 89	0 7 6 20	0 6 0 37	0 1 3 29	0 2 0 36			
1150	0 $4\frac{1}{2}$	0 12 5	0 4 $8\frac{1}{4}$		0 1 8	0 3 0		0 3	
1250	1 $7\frac{3}{4}$	1 11 0	1 0 7	0 17 0	0 1 7		1 0	0 3	0 $4\frac{1}{2}$
1350	1 $10\frac{1}{2}$	0 18 4 43	1 4 6 66	0 17 2 106	0 2 7 61	0 2 6 45	0 9 75	0 2 24	0 $3\frac{3}{4}$ 31
1450	1 5		1 15 8	0 15 6	0 4 $11\frac{1}{2}$	0 5 1	0 $6\frac{1}{4}$		
1550	1 $10\frac{1}{2}$	2 2 0 100	1 16 7 100	0 16 0 100	0 4 $3\frac{3}{4}$ 100	0 5 6 100	1 0 100	0 $8\frac{1}{4}$ 100	1 0 100
1600	4 $0\frac{1}{2}$								
1625	4 11						2 0		1 6
1650	5 6								
1675	4 6	5 10 0 250	3 6 0 184	2 17 0 345	0 11 0 256	0 14 0 254	3 0 300	1 8 182	1 8 125
1700	4 $9\frac{1}{2}$								
1720	4 $4\frac{1}{2}$								
1740	3 8	10 0 0 476	8 0 0 437	7 7 0 884	1 6 0 602	1 15 0 634	3 6 350	1 6 218	1 6 150
1760	3 $9\frac{3}{4}$	14 0 0 667	8 10 0 465	7 0 0 874	1 7 0 626	1 15 0 634	5 0 500	1 10 266	1 10 183
1780	4 $5\frac{1}{2}$								
1795	7 10	19 0 0 904	16 8 0 890	16 8 0 2000	1 18 0 882	5 8 0 1960	3 0 300	1 6 218	1 6 150

• The small figures denote the price in decimals, whereof those

Besides most of the old chronicles and historians, the following books were consulted, in constructing the 1299. The Sketch of the Establishments of this Kingdom, *temp.* Ed. III. *et seqq.* by J. Bree, 1791. Coll King William and Queen Mary, Lond. 1790, 4to. The 11th volume of the *Archæologia*. An Enquiry into th fol. Lond. by T. Longman, 1768. Dr. Smith's Wealth of Nations. Sir James Steuart's Political Œ

[*To face p.* 176.

t of Day Labour, in sterling Money, and also in Decimals, at different Periods, ; with the Depreciation of the Value of Money inferred therefrom. To which is of 50 Years, for the first 600 Years; and, during the present Century, at shorter

CLES AT DIFFERENT TIMES.

Cock.	Butter, per lb.	Cheese, per lb.	Ale, per Gallon.	Small Beer, per Gallon.	Mean Depreciation from these 12 Articles	Beef and Mutton, per lb.	Labour in Husbandry, per Day.	Depreciation of Money, according to the Price of					Mean Appreciation by Interpolation.	
								Wheat.	12 miscellaneous Articles.	Meat.	Day Labour.	Mean of all.	A. D.	
d.	*d.*	*d.*	*s. d.*	*d.*		*d. qr.*	*s. d.*							
					42			10	42			26	1050	26
							0 2						1100	34
$4\frac{1}{2}$													1150	43
$8\frac{1}{4}$							0 3						1200	51
31					56			100	56		75	77	1250	60
							0 $3\frac{3}{4}$						1300	68
0	5	2	0 $1\frac{1}{2}$	1		1 $0\frac{1}{2}$	0 4						1350	77
100	100	100	100	100	100			100	100	100	100	100	1400	83
			0 4	2		1 2	0 6						1450	88
6							0 $6\frac{1}{2}$						1500	94
			0 4	2									1550	100
8	$4\frac{1}{2}$	2	0 8	$2\frac{1}{2}$		1 $3\frac{1}{2}$	0 $7\frac{1}{2}$						1600	144
125	90	100	530	250	239			246	239	166	188	210	1650	188
			0 10	3									1675	210
			1 0	3		2 2	0 8						1700	238
6	9	$3\frac{1}{2}$	1 0	3		3 0	0 10						1720	257
150	180	175	800	300	434			197	434	266	250	287	1740	287
10	10	$5\frac{1}{2}$	1 2	3		4 2	0 11						1750	314
183	200	262	930	300	492			203	492	400	275	342	1760	342
							1 2						1770	384
6	$11\frac{1}{2}$	5	1 $2\frac{1}{2}$	$2\frac{1}{4}$		5 3	1 $5\frac{1}{4}$						1780	427
150	230	250	969	275	752			426	752	511	436	531	1790	496
													1795	531
													1800 nearly	562

those for the year 1550 may be taken for the integer, *viz.* 100.

ing the above table; *viz.* Bishop FLEETWOOD's *Chronicon Preciosum*, 1st and 2d edit. *Liber Garderobæ*, in Collection of Ordinances and Regulations of the Royal Household, in divers Reigns, from Edward III. to into the Prices of Wheat and other Provisions in England, from the Year 1000 to 1765, by Mr. COMBRUNE, ical Œconomy; and Dr. HENRY's History.

APPENDIX.

(§. 44.) Since the writing of the preceding Memoir, I have had an opportunity of examining three other scales, divided into inches, or equal parts, of considerable authority in this country, having been executed by the late Mr. J. BIRD. I have also compared the old standard in the Exchequer, of the time of Hen. VII. and which is considered to be the most ancient authority of this sort now subsisting: these observations, I flatter myself, the Royal Society will be desirous of possessing.

(§. 45.) The first of the abovementioned scales belonged to the late General ROY, and was purchased by him at Mr. SHORT's sale, the celebrated optician; it was used by him in his operations of measuring a base line on Hounslow Heath. (See Phil. Trans. Vol. LXXV.) It was originally the property of Mr. G. GRAHAM, has the name of JONATHAN SISSON engraven upon it, but is known to have been divided by Mr. BIRD, who then worked with old Mr. SISSON. It is 42 inches long, divided into tenths, with a vernier of 100 at one end, and of 50 at the other, giving the subdivisions of 500ths, and 1000ths, of an inch.

(§. 46.) The second is in the possession of ALEXANDER AUBERT, Esq. and formerly belonged to Mr. HARRIS, of the Tower; contains 60 inches, divided into 10ths, with a vernier, like that of the preceding. It is one inch broad, and 0,2 thick.

(§. 47.) The third was presented by ALEXANDER AUBERT, Esq. and the late Admiral CAMPBELL, Mr. BIRD's executors, to the Royal Society, in whose custody it now remains. It consists of a brass rod, 92,4 inches long, 0,57 inch broad, and

0,3 inch thick; bearing a scale of 90 inches, or equal parts, each subdivided into 10, with a vernier at the commencement, being a scale of 100 divisions to 101 tenths. *This* has been called Mr. BIRD'S own scale, *viz.* made for his own use; and was the instrument with which he is said to have laid off the divisions of his 8-feet mural quadrants. It is probable that Mr. BIRD made many more of these scales, now in the hands of private persons, (one of which, indeed, I saw at the President DE SARON'S, many years ago, at Paris,) but those have not come to my knowledge.

(§. 48.) In comparing General ROY'S (BIRD'S) scale with Mr. TROUGHTON'S, I found 42 inches of the former were = 42,00010 inches on TROUGHTON'S; (the thermometer 51°,7;) 36 inches were consequently = 36,00008.

		Inches.
And 12 inches on the 1st foot were equal to the 12 inches from 12 to 24 on TROUGHTON'S scale	— ,0003 =	11,9997
The 2d foot	+ ,0006	12,0006
The 3d foot	— ,0004	11,9996
The last foot	+ ,0006	12,0006
The mean foot, therefore, in General ROY'S scale, taken from four different feet, compared with TROUGHTON'S, between the 12th and 24th inch, is as 12 to		12,00012

That is, General ROY'S scale is longest on 1 foot by so much, and longer on 3 feet by - - ,00036

And the greatest probable error from the inequality in the divisions is about - - - - ,0005

And the mean probable error about - - ,0003

(§. 49.) Mr. AUBERT'S scale, compared with Mr. TROUGHTON'S, was as follows: 58 inches were equal to 57,9982 inches

on TROUGHTON's; (thermometer at 51°,0;) *viz.* Mr. BIRD's measure was shortest ,0018; or, shortest on 36 inches = ,0012,

	Inches.	
And 12 inches, or 1st foot, on Mr. AUBERT's - -	= 11,9999	on Mr. TROUGHTON's scale, from 6 inches to 18 inches; the thermometer being at 50°,0.
2d foot,	= 12,0005	
3d foot,	11,9996	
4th foot,	12,0019	
5th foot,	12,0006	
Therefore the mean foot is -	12,0005	

The greatest error in this scale appears to be about = ,0012
And the mean probable error - - = ,0006.

(§. 50.) The Royal Society's scale, compared, was as follows: 58 inches on Mr. BIRD's were equal to 57,99912 inches on Mr. TROUGHTON's: (thermometer 50°,5)

viz. Mr. BIRD's measure was shortest ,00088
Or shorter on 36 inches - - ,00054
32 inches on the same were equal to - 31,99967
viz. Mr. BIRD's was shortest by - ,00033
Or, on 36 inches, by - - ,00037
The mean of these two comparisons is ,00045
And, by so much, is Mr. BIRD's scale shorter, in three feet, than TROUGHTON's.

	Inches.	
And 12 inches, or 1st foot, of the Royal Society's scale, is -	= 12,00013	on TROUGHTON's scale; the thermometer at 51°.
2d foot of ditto	= 11,99957	
3d foot of ditto	= 12,00027	
4th foot of ditto	= 11,99990	
5th foot of ditto	= 12,00063	
6th foot of ditto	= 11,99823	
7th foot of ditto	= 12,00000	

The mean of these seven feet is = 11,99982
And the greatest error in these divisions = ,0008
And the mean probable error - = ,0004.

(§. 51.) Lest, however, it should be suspected, that Mr. TROUGHTON'S scale, with which I have made these comparisons, is not sufficiently correct for this apparent preference, I will now give the result of my examination of that scale, from one end to the other. I set the microscopes to an interval of nearly 6 inches, correctly speaking, it was 6,00013 inches, taken from a mean of the whole scale; and, comparing this interval successively, I found as follows.

	Inches.		Inches.		Error, or difference from the mean.
viz. from	0	to 6	= 6,00025 *		+ ,00012
	6	to 12	= 6,00013		,00000
	12	to 18	= 6,00020		+ ,00007
	18	to 24	= 6,00000		— ,00013
	24	to 30	= 6,00007		— ,00006
	30	to 36	= 6,00033		+ ,00020
	36	to 42	= 5,99980		— ,00033
	42	to 48	= 6,00020		+ ,00007
	48	to 54	= 6,00010		— ,00003
	54	to 60	= 6,00023		+ ,00010
Mean of all			= 6,00013.		

From whence it appears, that the greatest probable error, without a palpable mistake, in Mr. TROUGHTON'S divisions, is = ,00033 inch; against which, the chance is 9 to 1; and

* It is not pretended, that in this and the foregoing observations, the quantity of any interval can be determined to the precision of the one hundred thousandth part of an inch; but it *is* presumed, that with the assistance of the microscopes, the ten thousandth part of an inch becomes visible; and, as a mean is taken from 3 or 4 times reading off the micrometer at each trial, it has been deemed not unreasonable to set down the quantities to five places of decimals.

the mean probable error = ,00016; and that it is 4 to 1 the error doth not exceed $\frac{2}{10,000}$ inch.

This accuracy is about three times as great as that of Mr. BIRD's scales, and about equal to that of the divisions of my equatorial instrument, made by Mr. RAMSDEN, in 1791. See Phil. Trans. for 1793.

(§. 52.) I now proceed to the examination of the standard rod of Henry VII. which is an octangular brass bar, of about $\frac{1}{2}$ an inch in diameter, with one of the sides rudely divided, into halves, thirds, quarters, eighths, and sixteenths; and the first foot into inches. Each end is sealed with a crowned old English H, ( ), and from hence is concluded to be of the time of King Henry VII. *viz.* about 1490, but is now become wholly obsolete, since the introduction of the standard of Queen Elizabeth; but such as it is, I have thought proper to examine it, and find as follows:

	Inches.		Error, or difference on 3 Feet.
On this rod, $\frac{1}{3}$, or the 1st foot, is equal to	11,973 on TROUGHTON's.		
the 2d foot is -	11,948		
the 3d foot is -	12,047		
The mean foot is -	11,989	— ,011	— ,033
$\frac{1}{2}$ yard, or 18 inches -	= 17,946	— ,054	— ,108
$\frac{2}{3}$ yard, or 24 inches -	= 23,921	— ,079	— ,118
$\frac{3}{4}$ yard, or 27 inches - -	= 26,937	— ,063	— ,084
$\frac{7}{8}$ yard, or 31$\frac{1}{2}$ inches -	= 31,443	— ,057	— ,065
$\frac{15}{16}$ yard, or 33$\frac{3}{4}$ inches -	= 33,665	— ,085	— ,091
Entire yard, or 36 inches -	= 35,966	— ,034	— ,034
And the mean yard - -	= 35,924	Mean	— ,076

And, by so much, Mr. TROUGHTON's measure is longest.

And the probable error, in the divisions of this old standard, is about $\frac{1}{1000}$ inch.

(§. 53.) It may now be desirable to see the comparative lengths of these various standards and scales, reduced to one and the same measure, *viz.* Mr. TROUGHTON's.

	Inches on Troughton's.	Difference.	Probable error in divisions.
36 inches, on a mean, of Hen. VII. standard of 1490, are equal to	35,924	−,076	,03
——— of standard yard of Eliz. of 1588	36,015	+,015	,04
——— of standard ell of ditto, of 1588	36,016	+,016	,04
———*of yard-bed of Guildhall, about 1660	36,032	+,032	
———*of ell-bed of ditto, about 1660	36,014	+,014	
———*of standard of clock-makers' company, 1671	35,972	−,028	
———*of the Tower standard, by Mr. ROWLEY, about 1720	36,004	+,004	
——— of GRAHAM's standard, by SISSON, of 1742, *viz.* line E	=36,0013	+,0013	
——— of ditto, ditto, *viz.* line EXCH.	=35,9933	−,0067	
——— of Gen. ROY's (BIRD's) scale — all made, probably, between the years 1745 and 1760.	=36,00036	+,00036	,0003
——— of Mr. AUBERT's, ditto, ditto — all made, probably, between the years 1745 and 1760.	=35,99880	−,00120	,0006
——— of Royal Society's ditto, ditto — all made, probably, between the years 1745 and 1760.	=35,99955	−,00045	,0004
——— of Mr. BIRD's parliamentary standard, of 1758	=36,00023	+,00023	
——— of Mr. TROUGHTON's scale, in 1796	=36,00000	,00000	,0001.

From hence it appears, that the mean length of the standard yard, taken from the seven first instances in this table, agrees with the quantity assumed by Mr. BIRD, or Mr. TROUGHTON, to within $\frac{1}{1000}$ inch, but that the latter is the longest.

* These four quantities are taken from Mr. GRAHAM's account, in the Phil. Trans. Vol. XLII.

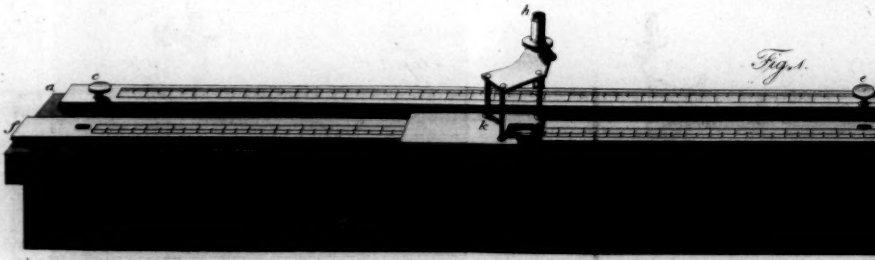


Fig. 1.

Fig. 2.

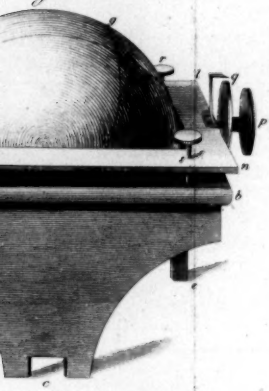
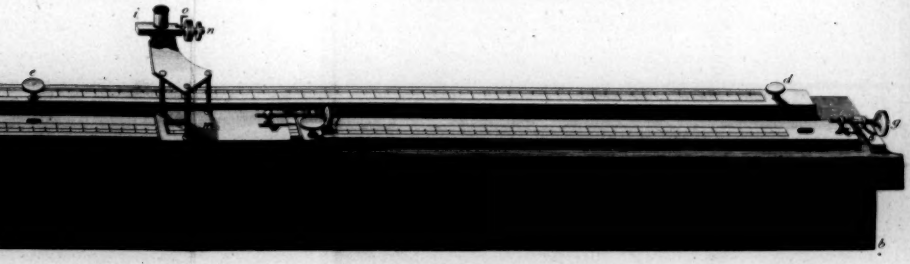
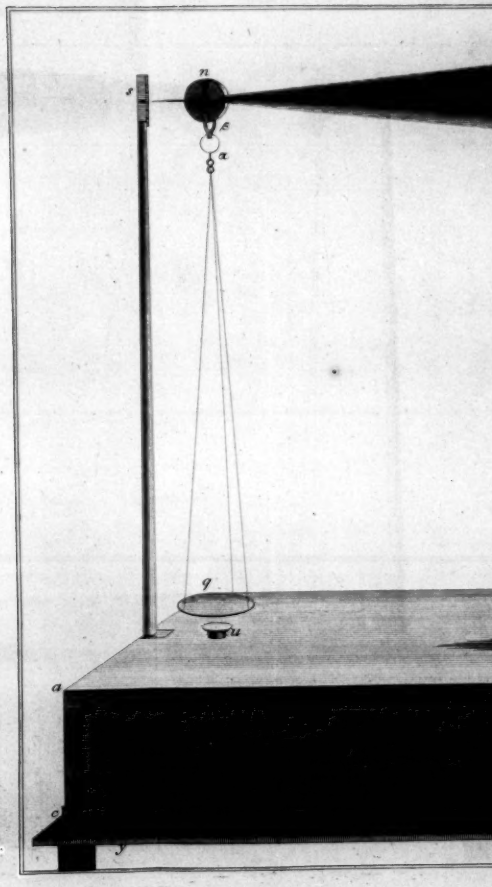


Fig. 3.

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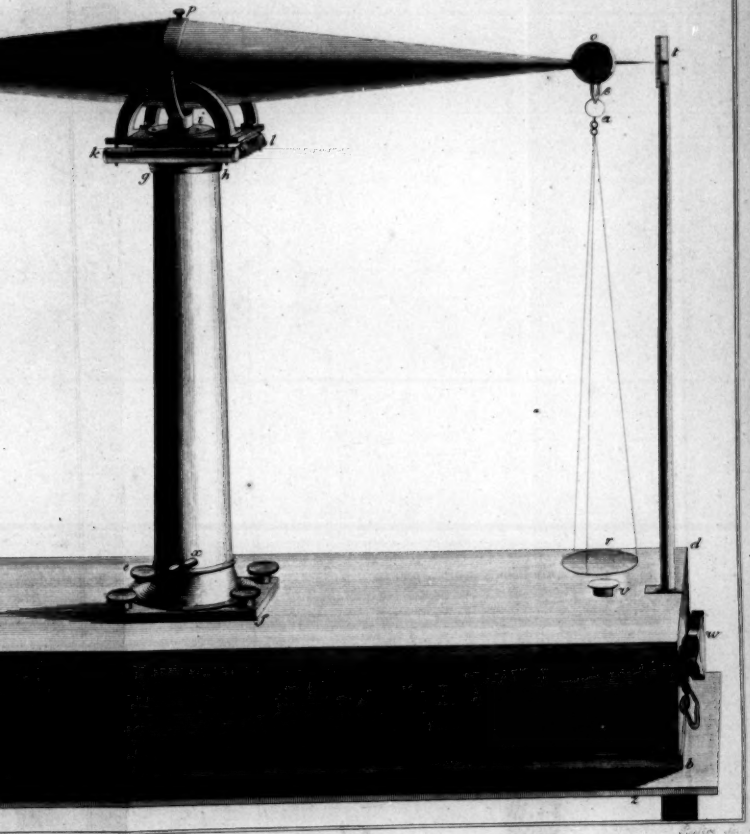
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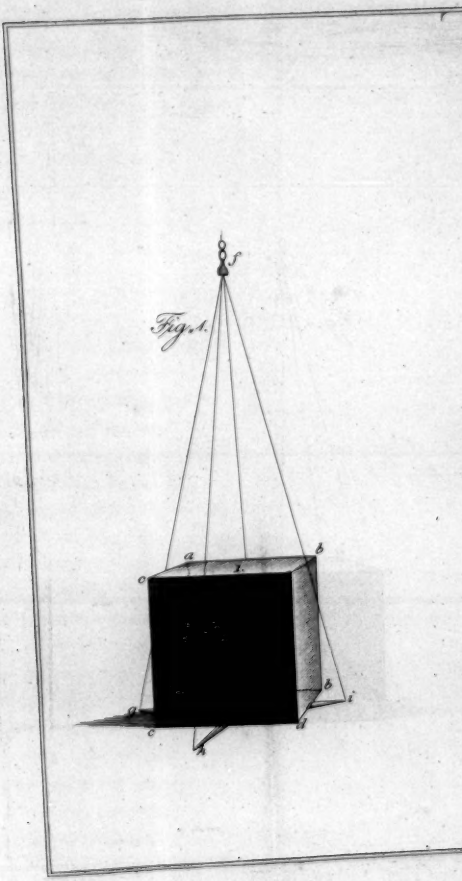
Fig. 1.
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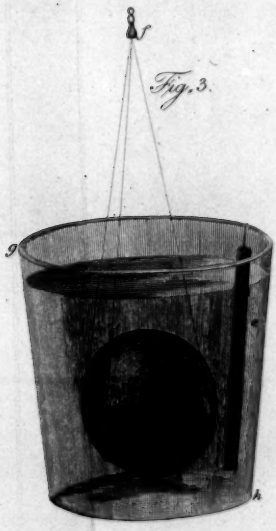

Fig. 3.

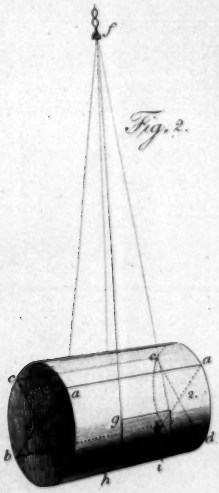

Fig. 2.

Basire Sc.

IX. *A new Method of computing the Value of a slowly converging Series, of which all the Terms are affirmative.* By the Rev. John Hellins, *F. R. S. and Vicar of* Potter's-Pury, *in* Northamptonshire. *In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. and Astronomer Royal.*

Read March 8, 1798.

REV. SIR, Potter's-Pury, February 8, 1798.

THAT several of the most curious and difficult problems in physical astronomy have hitherto been solved only by means of slowly converging series, is a truth which you are well acquainted with, and which may be seen in the works of the late learned EULER, and others, on that subject. Of this kind of series is the following, *viz.* $ax + bx^3 + cx^5 + dx^7 +$ &c. *ad infinitum*, when all the terms are affirmative, and a, b, c, &c. differ but little from each other, and x is but little less than 1; to obtain the value of which, to seven places of figures, by computing the terms as they stand, and adding them together, is a very laborious and tiresome operation; and therefore some easier method of obtaining it is very desirable. About five years ago, the consideration of this matter was recommended to me by Mr. Baron MASERES, (who not only employs his own leisure in explaining and improving the higher parts of the mathematics, but also encourages others who make the same laudable use of their leisure,) to whom I then communicated the method of computation explained in the paper inclosed in

this letter. As this method is general, for all slowly converging series of the form abovementioned, (which is generally allowed to be the most difficult,) I am induced to present it to you, requesting that, if it meets with your approbation, you will communicate it to the Royal Society.

I am,

Rev. Sir, *&c.*

JOHN HELLINS.

P. S. I need not observe to you, that it is not requisite to the summation of the series mentioned in this letter, that b should be less than a, c less than b, d less than c, *&c.* but only that the first, second, third, *&c.* differences of these coefficients should be a series of decreasing quantities: for, you well know, there are series of that form, which arise in physical astronomy, of which the coefficients are actually a diverging series, and yet the sum of the whole is a finite quantity. And the same thing is evident, from the bare inspection of the theorem which I shall presently use.

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1. The computing of the value of the series $ax + bx^{\frac{3}{2}} + cx^2 + dx^{\frac{5}{2}} +$ *&c. ad infinitum*, in which all the terms are affirmative, and the differences of the coefficients a, b, c, *&c.* are but small, though decreasing, quantities, and x is but little less than 1, is (as has been before observed) a laborious operation, and has engaged the attention of some eminent mathematicians, both at home and abroad, whose ingenious devices on the occasion entitle them to esteem. Of the several methods of

obtaining the value of this series, which have occurred to me, the easiest is that which I am now to describe, by which the business is reduced to the summation of two, three, or more series of this form, *viz.* $ax - bx^n + cx^{2n} - dx^{3n}$, *&c.* and one series of this form, *viz.* $px^n + qx^{2n} + rx^{3n} +$ *&c.* where n is $= 4, 8, 16, 32$, or some higher power of 2. The investigation of this method is as follows.

2. The series $ax + bx^2 + cx^3 + dx^4 + ex^5 + fx^6 +$ *&c.* is evidently equal to the sum of these two series, *viz.*

$$ax - bx^2 + cx^3 - dx^4 + ex^5 - fx^6, \text{ \&c.}$$
$$+ 2bx^2 + 2dx^4 + 2fx^6, \text{ \&c.}$$

of which, the value of the former is easily attainable, by the method so clearly explained, and fully illustrated, by Mr. Baron Maseres, in the Philosophical Transactions for the year 1777; and the latter, although it be of the same form with the series first proposed, yet has a great advantage over it, since it converges twice as fast. Upon this principle, then, we may proceed to resolve the series $2bx^2 + 2dx^4 + 2fx^6 + 2hx^8 + 2kx^{10} + 2mx^{12} +$ *&c.* into the two following:

$$2bx^2 - 2dx^4 + 2fx^6 - 2hx^8 + 2kx^{10} - 2mx^{12}, \text{ \&c.}$$
$$+ 4dx^4 + 4bx^8 + 4mx^{12}, \text{ \&c.}$$

where, again, the value of the one may easily be computed; and the other, although it be of the same form with the series at first proposed, yet converges four times as fast. And, in this manner we may go on, till we obtain a series of the same form with the series at first proposed, which shall converge 8, 16, 32, 64, *&c.* times as fast, and consequently a few terms of it will be all that are requisite.

An example, to illustrate this method, may be proper, which therefore is here subjoined.

3. Let it be proposed to find the value of the series $x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} +$ &c. *ad infinitum*, when $x = \frac{9}{10}$.

4. In order to obtain the sum of this series, with the less work, it will be requisite to compute a few of the initial terms, as they stand. For, if we begin the operation with computing the value of $x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4}$, &c. by the differential series before mentioned*, the values of D′, D″, D‴, &c. will be $\frac{1}{2}$, $\frac{1.2}{2.3}$, $\frac{1.2.3}{2.3.4}$, &c. respectively, *i. e.* $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. which is a series decreasing so very slowly, that the only advantage obtained by this transformation of the series is in the convergency of the powers of $\frac{x}{1+x}$ instead of the powers of x, which indeed is very great; for, x being $= \frac{9}{10}$, $\frac{x}{1+x}$ is $= \frac{9}{19}$; so that the new series $\frac{9}{19} + \frac{1}{2} \cdot \overline{\frac{9}{19}}^2 + \frac{1}{3} \cdot \overline{\frac{9}{19}}^3 + \frac{1}{4} \cdot \overline{\frac{9}{19}}^4 +$ &c. although $= \frac{9}{10} - \frac{1}{2} \cdot \overline{\frac{9}{10}}^2 + \frac{1}{3} \cdot \overline{\frac{9}{10}}^3 - \frac{1}{4} \cdot \overline{\frac{9}{10}}^4$, &c. yet converges more than seven times as swiftly†. But, if we begin the work by computing the first eight terms of the series, as they stand, and then compute the value of $\frac{x}{9} - \frac{x^2}{10} + \frac{x^3}{11} - \frac{x^4}{12} +$, &c. *ad infinitum*, by the same theorem, the values of D′, D″, D‴, &c. will be $\frac{1}{9.10}$, $\frac{2}{9.10.11}$, $\frac{2.3}{9.10.11.12}$, &c. which is a series decreasing, for a great number

* The theorem best adapted to this business is the following; *viz.* $ax - bx^2 + cx^3 - dx^4$, &c. $= \frac{ax}{1+x} + \frac{D'x^2}{(1+x)^2} + \frac{D''x^3}{(1+x)^3} + \frac{D'''x^4}{(1+x)^4} +$ &c. D′ being $= a - b$, D″ $= a - 2b + c$, D‴ $= a - 3b + 3c - d$, &c.

See *Scriptores Logarithmici*, Vol. III. p. 290, where *b*, *c*, *d*, &c. denote the same quantities that *a*, *b*, *c*, &c. do here.

† $\frac{9}{19}$ is $= 0.4736842$, and $\overline{\frac{9}{10}}^7$ is $= 0.4782969$.

of its terms, much more swiftly than the powers of $\frac{2}{10}$, and, in the first seven terms, much more swiftly than the powers of $\frac{2}{19}$. The value of the series $\frac{1}{9} \cdot \frac{2}{10} - \frac{1}{10} \cdot \overline{\frac{2}{10}}^2 + \frac{2}{11} \cdot \overline{\frac{2}{10}}^3 - \frac{1}{13} \cdot \overline{\frac{2}{10}}^4$, *&c.* is therefore = the series $\frac{1}{9} \cdot \frac{2}{19} + \frac{1}{9.10} \cdot \overline{\frac{2}{19}}^2 + \frac{2}{9.10.11} \cdot \overline{\frac{2}{19}}^3 + \frac{2.3}{9.10.11.12} \cdot \overline{\frac{2}{19}}^4 +$ *&c.* the first seven terms of which converge above fourteen times as swiftly as the other; or, in other words, the first seven terms of it will give a result much nearer the truth than a hundred terms of the other. And if, instead of the first eight terms of the proposed series, the first twenty-four terms were computed, as they stand, and then the value of the series $\frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28}$, *&c.* by its equivalent, $\frac{1}{25} \frac{x}{1+x} + \frac{1}{25.26} \cdot \frac{x^2}{(1+x)^2} + \frac{2}{25.26.27} \cdot \frac{x^3}{(1+x)^3} + \frac{2.3}{25.26.27.28} \cdot \frac{x^4}{(1+x)^4} +$ *&c.* the rapid decrease of the coefficients $\frac{1}{25}, \frac{1}{25.26}, \frac{2}{25.26.27}$, *&c.* compounded with the decrease of the powers of $\frac{x}{1+x}$, (in the present case = the powers of $\frac{2}{19}$,) produces such a very swiftly converging series, that eight terms of it will give the result true to eleven places of decimals.

It may be further remarked, for the sake of my less experienced readers, (for whose information this article is chiefly intended,) that the second term of the last series is produced by multiplying the first by $\frac{1}{26} \cdot \frac{x}{1+x}$; the third, by multiplying the second by $\frac{2}{27} \cdot \frac{x}{1+x}$; the fourth, by multiplying the third by $\frac{3}{28} \cdot \frac{x}{1+x}$; and so on. If, therefore, the first term be called P, the second Q, the third R, the fourth S, *&c.* we shall have,

$$P = \frac{1}{25} \cdot \frac{x^2}{1+x^2},$$

$$Q = \frac{P}{26} \cdot \frac{x}{1+x},$$

$$R = \frac{2Q}{27} \cdot \frac{x}{1+x},$$

$$S = \frac{3R}{28} \cdot \frac{x}{1+x}; \text{ \&c.}$$

which form is well adapted to arithmetical calculation. And, that a similar form for that purpose may always be obtained, when the coefficients of the proposed series are the reciprocals of any arithmetical progression, has been shewn by Mr. SIMPSON, in his Mathematical Dissertations, p. 64.

Having premised these observations, I now proceed to the arithmetical work; in which I shall use, and explain, such other devices as have occurred to me for facilitating it.

5. Since the work of computing the value of the proposed series is so much shortened, by first finding the sum of a moderate number of initial terms, and then resolving the remaining terms into several series, in the manner described in Art. 2, I will begin with computing the first twenty-four terms of it; and, to facilitate this part of the work, I will separate these terms into three parts; *viz.*

$$x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} + \frac{x^8}{8},$$

$$+ \frac{x^9}{9} + \frac{x^{10}}{10} + \frac{x^{11}}{11} + \frac{x^{12}}{12} + \frac{x^{13}}{13} + \frac{x^{14}}{14} + \frac{x^{15}}{15} + \frac{x^{16}}{16},$$

$$+ \frac{x^{17}}{17} + \frac{x^{18}}{18} + \frac{x^{19}}{19} + \frac{x^{20}}{20} + \frac{x^{21}}{21} + \frac{x^{22}}{22} + \frac{x^{23}}{23} + \frac{x^{24}}{24},$$

which are evidently equal to

$$x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} + \frac{x^8}{8},$$

$$x^9\left(\frac{x}{9} + \frac{x^2}{10} + \frac{x^3}{11} + \frac{x^4}{12} + \frac{x^5}{13} + \frac{x^6}{14} + \frac{x^7}{15} + \frac{x^8}{16}\right),$$

$$x^{16}\left(\frac{x}{17} + \frac{x^2}{18} + \frac{x^3}{19} + \frac{x^4}{20} + \frac{x^5}{21} + \frac{x^6}{22} + \frac{x^7}{23} + \frac{x^8}{24}\right);$$

which three parts, for the sake of reference, may be called A, Bx^8, and Cx^{16}. It is evident also, that the numerical values of the first eight powers of x are all that are required for finding the sum of these twenty-four terms of the series proposed: for $Bx^8 + Cx^{16}$ is $= x^8 (B + Cx^8)$; and this product $+ A =$ the sum required*.

The numerical values of the first eight powers of x are these:

$x = 0{\cdot}9,$
$x^2 = 0{\cdot}81,$
$x^3 = 0{\cdot}729,$
$x^4 = 0{\cdot}6561,$
$x^5 = 0{\cdot}59049,$
$x^6 = 0{\cdot}531441,$
$x^7 = 0{\cdot}4782969,$
$x^8 = 0{\cdot}43046721;$

from which values of the powers of x, the values of A, B, and C, continued to twelve places of decimals, are easily obtained, and are as follow.

* The same thing might have been obtained from the first four powers of x, or from the first six powers of x; as might likewise the sum of 32, 36, 40, or any other number of terms that is a multiple of 4 or 6. But the number 8 is here chosen, for the sake of a subsequent use that will be made of the value of x^8.

$x = 0.9$

$\frac{x^2}{2} = 0.405$

$\frac{x^3}{3} = 0.243$

$\frac{x^4}{4} = 0.16402,5$

$\frac{x^5}{5} = 0.11809,8$

$\frac{x^6}{6} = 0.08857,35$

$\frac{x^7}{7} = 0.06832,81285,71$

$\frac{x^8}{8} = 0.05380,84012,50$

The sum is $2.04083,30298,21 = A.$

$\frac{x}{9} = 0.1$

$\frac{x^2}{10} = 0.081$

$\frac{x^3}{11} = 0.06627,27272,73$

$\frac{x^4}{12} = 0.05467,50000,00$

$\frac{x^5}{13} = 0.04542,23076,92$

$\frac{x^6}{14} = 0.03796,00714,29$

$\frac{x^7}{15} = 0.03188,64600,00$

$\frac{x^8}{16} = 0.02690,42006,25$

The sum is $0.44412,07670,19 = B.$

$\frac{x}{17} = 0{\cdot}05294,11764,71$

$\frac{x^2}{18} = 0{\cdot}04500,00000,00$

$\frac{x^3}{19} = 0{\cdot}03836,84210,53$

$\frac{x^4}{20} = 0{\cdot}03280,50000,00$

$\frac{x^5}{21} = 0{\cdot}02811,85714,29$

$\frac{x^6}{22} = 0{\cdot}02415,64090,91$

$\frac{x^7}{23} = 0{\cdot}02079,55173,91$

$\frac{x^8}{24} = 0{\cdot}01793,61337,50$

The sum is $0{\cdot}26012,12291,85 = C$.

We may now quickly find the sum of the first twenty-four terms of the series proposed, by two multiplications by x^8, and two additions, as was pointed out in the former part of this article: but, since the labour of these two separate multiplications may be saved, I shall now proceed to compute the value of the remaining terms of the proposed series.

6. The remaining terms are $\frac{x^{25}}{25} + \frac{x^{26}}{26} + \frac{x^{27}}{27} + \frac{x^{28}}{28} + \frac{x^{29}}{29} + \frac{x^{30}}{30} +$ *&c. ad infinitum*, which, being disposed according to the form mentioned in Art. 2, are evidently equal to $x^{24} \times$ these two series $\begin{cases} \frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28} + \frac{x^5}{29} - \frac{x^6}{30}, \text{ \&c.} \\ * + \frac{2x^2}{26} \quad * + \frac{2x^4}{28} \quad * + \frac{2x^6}{30} +, \text{ \&c.}; \end{cases}$ the value of the first of which is very easily attainable, in the manner shewn above, in Art. 4. The arithmetical work will stand thus:

$$P = \frac{1}{25} \times \frac{9}{19} = 0{\cdot}01894,78684,2$$

$$Q = \frac{P}{26} \times \frac{9}{19} = 0{\cdot}00034,51949,7$$

$$R = \frac{2Q}{27} \times \frac{9}{19} = 0{\cdot}00001,21121,0$$

$$S = \frac{3R}{28} \times \frac{9}{19} = 0{\cdot}00000,06147,1$$

$$T = \frac{4S}{29} \times \frac{9}{19} = 0{\cdot}00000,00401,6$$

$$U = \frac{5T}{30} \times \frac{9}{19} = 0{\cdot}00000,00031,7$$

$$V = \frac{6U}{31} \times \frac{9}{19} = 0{\cdot}00000,00002,9$$

$$W = \frac{7V}{32} \times \frac{9}{19} = 0{\cdot}00000,00000,3$$

The sum of these terms is 0·01930,53338,5, which, for the sake of reference, call a. Then we have $ax^{24} = x^{24} \times : \frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28} + \frac{x^5}{29} - \frac{x^6}{30}$, *&c.*

7. The part of the proposed series which now remains to be computed, is $x^{24} \times : \frac{x^{25}}{26} + \frac{x^{26}}{28} + \frac{x^{27}}{30} + \frac{x^{28}}{32} + \frac{x^{29}}{34} + \frac{x^{30}}{36} +$ *&c. ad infinitum*, or $x^{24} \times : \frac{x^1}{13} + \frac{x^2}{14} + \frac{x^3}{15} + \frac{x^4}{16} + \frac{x^5}{17} + \frac{x^6}{18} +$ *&c. ad infinitum*, which may also be divided into two series, in the manner above shewn; but, to obtain a swifter convergency in the next, as well as the succeeding applications of the differential series, it will be convenient first to compute the value of four terms at the beginning of this series; which we may quickly do, since all the powers of x requisite in that part of the calculation are ready at hand, being set down in Art. 5. The work will stand thus:

$$\frac{x^{13}}{13} = 0{\cdot}06230,76923,1$$
$$\frac{x^{14}}{14} = 0{\cdot}04686,42857,1$$
$$\frac{x^{15}}{15} = 0{\cdot}03542,94000,0$$
$$\frac{x^{16}}{16} = 0{\cdot}02690,42006,3;$$

And the sum is 0·17150,35786,5, which call D. Then will $Dx^{24} = x^{24} \times \left(\frac{x^{13}}{13} + \frac{x^{14}}{14} + \frac{x^{15}}{15} + \frac{x^{16}}{16}\right)$.

8. It now remains to find the value of $x^{24} \times : \frac{x^{17}}{17} + \frac{x^{18}}{18} + \frac{x^{19}}{19} + \frac{x^{20}}{20} + \frac{x^{21}}{21} + \frac{x^{22}}{22} +$ *&c. ad infinitum*, or of its equal, $x^{25} \times$ the sum of these two series,

$$\frac{x^{5}}{17} - \frac{x^{6}}{18} + \frac{x^{7}}{19} - \frac{x^{8}}{20} + \frac{x^{9}}{21} - \frac{x^{10}}{22}, \&c.$$
$$* + \frac{2x^{6}}{18} \quad * + \frac{2x^{8}}{20} \quad * + \frac{2x^{10}}{22} + \&c.$$

Here, again, the value of the first series may easily be computed, by the theorem referred to in Art. 4, it being $= \frac{1}{17} \cdot \frac{x^5}{1+xx} + \frac{1}{17.18} \cdot \frac{x^4}{(1+xx)^2} + \frac{1.2}{17.18.19} \cdot \frac{x^6}{(1+xx)^3} + \frac{1.2.3}{17.18.19.20} \cdot \frac{x^8}{(1+xx)^4} + \&c.$ in numbers,

$$P = \frac{1}{17} \times \frac{81}{181} = 0{\cdot}02632,43418,9$$

$$Q = \frac{P}{18} \times \frac{81}{181} = 0{\cdot}00065,44725,9$$

$$R = \frac{2Q}{19} \times \frac{81}{181} = 0{\cdot}00003,08300,6$$

$$S = \frac{3R}{20} \times \frac{81}{181} = 0{\cdot}00000,20695,3$$

$$T = \frac{4S}{21} \times \frac{81}{181} = 0{\cdot}00000,01764,1$$

$$U = \frac{5T}{22} \times \frac{81}{181} = 0{\cdot}00000,00179,4$$

$$V = \frac{6U}{23} \times \frac{81}{181} = 0{\cdot}00000,00020,9$$

$$W = \frac{7V}{24} \times \frac{81}{181} = 0{\cdot}00000,00002,7$$

$$X = \frac{8W}{25} \times \frac{81}{181} = 0{\cdot}00000,00000,4$$

And the sum of these terms is $0{\cdot}02701,19108,2 = C$.

9. To find the value of $x^{25} \times : \frac{2x^{8}}{18} + \frac{2x^{9}}{20} + \frac{2x^{10}}{22} + \frac{2x^{11}}{24} + \&c.$ *ad infinitum*, or its equal, $x^{25} \times : \frac{x^{8}}{9} + \frac{x^{9}}{10} + \frac{x^{10}}{11} + \frac{x^{11}}{12} + \&c.$ *ad infinitum*, which is all that now remains of the proposed series, it will be expedient, first, to compute the two initial terms of the series, and then to separate the remaining terms of it into two parts, as has been done in the preceding articles. These two terms are

$$\frac{x^{8}}{9} = 0{\cdot}07290,00000,0$$

$$\frac{x^{9}}{10} = 0{\cdot}04304,67210,0$$

And their sum is $0{\cdot}11594,67210,0 = E$.

10. There now remains, of the proposed series, $x^{35} \times : \frac{x^{35}}{11} + \frac{x^{36}}{12} + \frac{x^{30}}{13} + \frac{x^{34}}{14} + \frac{x^{28}}{15} + \frac{x^{32}}{16} +$ *&c. ad infinitum*, $= x^{46} \times$ the sum of these two series,

$$\frac{x^4}{11} - \frac{x^8}{12} + \frac{x^{12}}{13} - \frac{x^{16}}{14} + \frac{x^{20}}{15} - \frac{x^{24}}{16}, \text{ \&c.}$$

$$* + \frac{2x^8}{12} \quad * + \frac{2x^{16}}{14} \quad * + \frac{2x^{24}}{16} + \text{ \&c.}$$

Here, likewise, the value of the series which has the signs + and — alternately, is easily obtained by a swiftly converging series, the terms of which are set down here below, the decimal value of $\frac{x^4}{1+x^4} = \frac{6561}{16561}$ being used in the calculation, for the sake of facility.

$$P = \frac{1}{11} \times 0{\cdot}39617,17287,6 = 0{\cdot}03601,56117,1$$

$$Q = \frac{P}{12} \times 0{\cdot}39617,17287,6 = 0{\cdot}00118,90306,0$$

$$R = \frac{2Q}{13} \times 0{\cdot}39617,17287,6 = 0{\cdot}00007,24708,2$$

$$S = \frac{3R}{14} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,61523,3$$

$$T = \frac{4S}{15} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,06499,7$$

$$U = \frac{5T}{16} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,00804,7$$

$$V = \frac{6U}{17} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,00112,5$$

$$W = \frac{7V}{18} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,00017,3$$

$$X = \frac{8W}{19} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,00002,9$$

$$Y = \frac{9X}{20} \times 0{\cdot}39617,17287,6 = 0{\cdot}00000,00000,5$$

The sum of these terms is $0{\cdot}03728,40092,2 = \gamma$.

11. The part now remaining, of the proposed series, is $x^{45} \times : \frac{2x^3}{12} + \frac{2x^{16}}{14} + \frac{2x^{24}}{16} + \frac{2x^{35}}{18} + \frac{2x^{40}}{20} + \frac{2x^{48}}{22} + \&c.$ *ad infinitum*, which may very conveniently be resolved into $x^{45} \times$ these two series $\begin{cases} \frac{1}{6} + \frac{x^3}{7} - \frac{x^{16}}{8} + \frac{x^{24}}{9} - \frac{x^{35}}{10} + \frac{x^{40}}{11} - \frac{x^{48}}{12}, \&c. \\ * + \frac{2x^{16}}{8} \quad * + \frac{2x^{35}}{10} \quad * + \frac{2x^{48}}{12} + \&c. \end{cases}$ the value of the first of which will quickly be obtained, by means of the theorem which has been repeatedly used in the preceding operations. The terms of it are as below, in which the decimal value of $\frac{x^3}{1+x^3} = \frac{43046721}{143046721}$ is used, for the reason mentioned in the preceding article.

$$\frac{1}{6} = \quad - \quad - \quad \cdot \quad 0{\cdot}16666,66667$$

$$P = \frac{1}{7} \times 0{\cdot}30092,77018 = 0{\cdot}04298,96717$$

$$Q = \frac{P}{8} \times 0{\cdot}30092,77018 = 0{\cdot}00161,70979$$

$$R = \frac{2Q}{9} \times 0{\cdot}30092,77018 = 0{\cdot}00010,81399$$

$$S = \frac{3R}{10} \times 0{\cdot}30092,77018 = 0{\cdot}00000,97627$$

$$T = \frac{4S}{11} \times 0{\cdot}30092,77018 = 0{\cdot}00000,10683$$

$$U = \frac{5T}{12} \times 0{\cdot}30092,77018 = 0{\cdot}00000,01339$$

$$V = \frac{6U}{13} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00186$$

$$W = \frac{7V}{14} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00028$$

$$X = \frac{8W}{15} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00005$$

$$Y = \frac{9X}{16} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00001$$

The sum of these terms is $0{\cdot}21139,25631 = \delta$.

12. There now remains, of the proposed series, only $x^{42} \times : \frac{2x^{16}}{8} + \frac{2x^{23}}{10} + \frac{2x^{30}}{12} + \frac{2x^{37}}{14} + \&c.$ *ad infinitum*, the value of which, or of its equal, $x^{42} \times : \frac{x^{16}}{4} + \frac{x^{23}}{5} + \frac{x^{30}}{6} + \frac{x^{37}}{7} + \&c.$ may easily be obtained without any transformation, since the powers of x^{16} (= the powers of 0·18530,20188,9) decrease swifter than the powers of $\frac{1}{3}$. The numerical values of these terms are as below.

$$\frac{x^{16}}{4} = 0{\cdot}04632,55047$$
$$\frac{x^{23}}{5} = 0{\cdot}00686,73676$$
$$\frac{x^{30}}{6} = 0{\cdot}00106,04476$$
$$\frac{x^{37}}{7} = 0{\cdot}00016,84312$$
$$\frac{x^{44}}{8} = 0{\cdot}00002,73093$$
$$\frac{x^{51}}{9} = 0{\cdot}00000,44982$$
$$\frac{x^{58}}{10} = 0{\cdot}00000,07502$$
$$\frac{x^{65}}{11} = 0{\cdot}00000,01264$$
$$\frac{x^{72}}{12} = 0{\cdot}00000,00215$$
$$\frac{x^{79}}{13} = 0{\cdot}00000,00037$$
$$\frac{x^{86}}{14} = 0{\cdot}00000,00006$$
$$\frac{x^{93}}{15} = 0{\cdot}00000,00001$$

The sum of these terms is 0·05445,44611 = F.

13. The values of the several parts, into which the proposed series has been resolved, being now so far obtained that we have only to multiply each by its proper factor, *viz.* the numerical value of x^3, x^{16}, x^{25}, &c. and add the products together, to get the sum of it; this, therefore, is now to be performed. And, in this part of the calculation, several multiplications may be saved, and no larger factor than x^3 be used, by attending to the method described by Sir ISAAC NEWTON, in his Tract *De Analysi per Æquationes infinitas*; p. 10. of Mr. JONES's edition of Sir ISAAC's Tracts; or p. 270. Vol. I. of Bishop HORSLEY's edition of all his works. The manner in which this is to be done will appear, by collecting the several parts from the preceding Articles, and exhibiting them in one view, thus:

$A + Bx^3 + Cx^{16} + \overline{\alpha + D \times x^{34}} + \overline{\zeta + E \times x^{25}} + \gamma x^{40} + \overline{\delta + F \times x^{49}}$ = the sum of the proposed series. Now,

1st. Calling $\delta + F$, x', and multiplying by x^3, we have $x' x^3 = 0{\cdot}26584,70242 \times 0{\cdot}43046,721 = 0{\cdot}11443,84267,9$.

2dly. Putting $\gamma + x' x^3 = x''$, and multiplying by x^3, we get $x'' x^3 = 0{\cdot}15172,24360,1 \times 0{\cdot}43046,721 = 0{\cdot}06531,15337,2$.

3dly. Putting $\zeta + E + x'' x^3 = x'''$, and multiplying by x^3, we get $x''' x^3 = 0{\cdot}20827,01655,4 \times 0{\cdot}43046,721 = 0{\cdot}08965,34770,9$.

4thly. Putting $\alpha + D + x''' x^3 = x^{iv}$, and multiplying by x^3, we get $x^{iv} x^3 = 0{\cdot}28046,43895,9 \times 0{\cdot}43046,721 = 0{\cdot}12073,07232,90$.

5thly. Putting $C + x^{iv} x^3 = x^v$, and multiplying by x^3, we get $x^v x^3 = 0{\cdot}38085,19524,75 \times 0{\cdot}43046,721 = 0{\cdot}16394,42774,05$.

6thly. Putting $B + x^v x^3 = x^{vi}$, and multiplying by x^3, we get $x^{vi} x^3 = 0{\cdot}60806,50444,24 \times 0{\cdot}43046,721 = 0{\cdot}26175,20631,73$.

Lastly, to this product add A - $= 2{\cdot}04083,30298,21$, and we have the value of the series proposed $= 2{\cdot}30258,50929,94$, which is true to twelve places of figures.

14. It may not be improper to remark, that this degree of accuracy is much greater than is requisite, even in astronomy; for which, as well as for most other purposes, six or seven places of figures are sufficient: and, to that degree of exactness the value of the proposed series might have been obtained, by less than a fourth part of the labour that has been taken above. But it was my intention to show, that the value of a very slowly converging series, of which all the terms are affirmative, may, by the method now described, be computed to ten or twelve places of figures, in the space of a few hours.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1797.

1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Jan. 1	45	8 0	45	52	29,74	88		S	1	Cloudy.
	47	2 0	47	54	29,83	87		S	1	Cloudy.
2	43	8 0	43	53	29,91	89	0,025	S	2	Cloudy.
	45	2 0	45	56	29,98	87		SSE	1	Cloudy.
3	36	8 0	38	52	30,20	87		E	1	Fair.
	40	2 0	39	54	30,28	87		ESE	1	Cloudy.
4	34	8 0	36	52	30,33	87		E	1	Cloudy.
	39	2 0	39	54	30,35	86		E	1	Cloudy.
5	37	8 0	37	52	30,40	86		NE	1	Cloudy.
	39	2 0	39	53	30,40	81		NE	1	Cloudy.
6	32	8 0	32	51	30,43	81		NE	1	Cloudy.
	34	2 0	34	53	30,45	80		NE	1	Cloudy.
7	30	8 0	30	49	30,44	84		NE	1	Cloudy.
	31	2 0	31	51	30,45	85		NE	1	Cloudy.
8	29	8 0	29	48,5	30,47	82		NE	1	Cloudy.
	30	2 0	30	50	30,48	82		NE	1	Cloudy.
9	25	8 0	25	47	30,50	84		NE	1	Cloudy.
	26	2 0	26	48,5	30,46	85		NE	1	Cloudy.
10	25	8 0	31	46	30,18	87		NE	1	Snow.
	34	2 0	33	48	30,05	85		ENE	1	Cloudy.
11	26	8 0	27	45	29,80	80		E	1	Cloudy.
	30	2 0	29	48	29,70	79		ESE	2	Cloudy.
12	28	8 0	31	45	29,70	88		NE	1	Snow.
	32	2 0	32	47,5	29,84	85		NW	1	Snow.
13	25	8 0	25	45	30,03	80		W	1	Cloudy.
	42	2 0	36	47	29,84	86		SSE	2	Rain.
14	36	8 0	48	48	29,52	90	0,272	SW	2	Cloudy.
	49	2 0	42	50	29,61	88		NW	1	Cloudy.
15	35	8 0	36	48	29,78	88	0,308	SW	1	Cloudy.
	35	2 0	34	50	29,73	88		E	1	Rain.
16	32	8 0	33	48	30,01	87	0,244	NE	1	Cloudy.
	36	2 0	36	51	30,16	81		NE	1	Fine.

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for January, 1797.

1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Jan. 17	25	8 0	26	48	30,39	87		SW	1	Fair.
	35	2 0	35	51	30,40	85		SSW	1	Fine.
18	31	8 0	35	48	30,34	85		SSW	1	Cloudy.
	39	2 0	38	50	30,29	87		SSW	1	Rain.
19	34	8 0	35	48	30,31	90	0,033	SSW	1	Foggy.
	40	2 0	39	51	30,35	90		SSW	1	Cloudy.
20	39	8 0	43	50	30,33	90		SSW	1	Cloudy.
	49	2 0	49	54	30,27	89		SSW	1	Cloudy.
21	47	8 0	47	53	30,26	90		SSW	1	Cloudy.
	48	2 0	48	55	30,20	86		SW	1	Cloudy.
22	43	8 0	43	54	30,16	87		SW	2	Cloudy.
	44	2 0	43	55,5	30,07	85		SSW	2	Cloudy.
23	43	8 0	43	53	30,07	87		S	2	Cloudy.
	43	2 0	43	55	30,04	85		S	2	Cloudy.
24	39	8 0	41	53	29,92	86		S	2	Fair.
	44	2 0	43	54,5	29,98	85		W	2	Cloudy.
25	30	8 0	30	52	30,24	86		SSW	1	Fair.
	38	2 0	38	54	30,24	83		SSW	1	Fine.
26	31	8 0	35	51,5	30,11	86		E	1	Cloudy.
	42	2 0	42	54,5	30,03	79		SE	1	Cloudy.
27	33	8 0	35	51	30,00	86		SE	1	Fair.
	45	2 0	42	54	29,89	87		SSE	1	Fair.
28	42	8 0	43	52	29,53	89	0,038	S	2	Rain. [Much wind last night.
	44	2 0	39	54	29,61	84		S	2	Cloudy.
29	36	8 0	43	51	29,92	87	0,040	SW	2	Rain.
	44,5	2 0	43	55	30,07	74		WNW	2	Fine.
30	38	8 0	42	52	30,17	85		WSW	2	Fair.
	49	2 0	49	55	30,16	82		WSW	2	Cloudy.
31	45	8 0	45,5	53	29,70	87		S	2	Rain. [Much wind last night.
	47	2 0	44	56	29,73	69		W	2	Fair.

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for February, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	o										
Feb. 1	36	7	o	44	53	29,96	88	0,068	SW	2	Cloudy.
	50	2	o	50	56	29,99	86		WNW	2	Cloudy.
2	46	7	o	46	54,5	30,27	87		WSW	2	Cloudy.
	50	2	o	50	57	30,32	85		SW	2	Cloudy.
3	45	7	o	45,5	56	30,37	82		SW	1	Cloudy.
	49	2	o	49	57	30,37	79		SSW	1	Cloudy.
4	37,5	7	o	38,5	56	30,36	85		NW	1	Cloudy.
	40	2	o	40	57	30,41	84		NNW	1	Cloudy.
5	35	7	o	35	55	30,50	84		E	1	Cloudy.
	36	2	o	36	55	30,54	85		E	1	Cloudy.
6	32	7	o	32,5	52	30,53	85		SE	1	Cloudy.
	37	2	o	37	54	30,55	82		NW	1	Cloudy.
7	32	7	o	33	52	30,55	82		SE	1	Cloudy.
	35	2	o	35	53	30,55	85		SE	1	Cloudy.
8	31	7	o	31	52	30,47	85		SW	1	Cloudy.
	35	2	o	32	52	30,48	83		SW	1	Cloudy.
9	32	7	o	35	51	30,59	82		SW	1	Cloudy.
	41	2	o	41	53	30,61	80		SSE	1	Fair.
10	31,5	7	o	31,5	52	30,62	83		SE	1	Cloudy.
	33	2	o	33	53	30,57	82		E	1	Cloudy.
11	29	7	o	30	51	30,44	84		S	1	Cloudy.
	36	2	o	36	52	30,33	85		SSE	1	Cloudy.
12	35	7	o	37	51	30,08	87		SW	1	Cloudy.
	46	2	o	46	52	30,04	85		SSW	1	Fair.
13	38	7	o	41	51	29,81	87	0,023	S	2	Rain.
	45	2	o	45	53	29,62	81		S	2	Cloudy.
14	36	7	o	36	51	29,44	84	0,128	WSW	2	Fair.
	42	2	o	42	53	29,37	76		WSW	2	Cloudy.
15	29	7	o	29,5	51	29,82	79		W	1	Fair.
	37	2	o	37	52	29,98	69		NW	2	Fine.
16	25	7	o	25	50	30,36	78		WNW	1	Fine.
	39	2	o	39	51	30,41	72		W	1	Fair.

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for February, 1797.

1797	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Feb. 17	27,5	7	0	27,5	50	30,46	78		E	1	Fine.
	41,5	2	0	41,5	54	30,40	73		ESE	1	Fine.
18	29	7	0	30	51	30,40	82		E	1	Fair.
	46	2	0	46	54	30,40	70		SSE	1	Fine.
19	29	7	0	29	51	30,48	81		ENE	1	Fair.
	44	2	0	43,5	54,5	30,50	80		S	1	Fine.
20	28	7	0	28	52	30,43	81		NE	1	Fair.
	44	2	0	44	55	30,37	77		ENE	1	Fine.
21	26	7	0	26	51,5	30,40	82		ENE	1	Foggy.
	46	2	0	46	55	30,40	82		E	1	Fine.
22	30	7	0	33	51,5	30,42	83		E	1	Fair.
	49,5	2	0	49,5	56	30,44	78		SE	1	Fine.
23	31	7	0	31	51	30,42	84		NE	1	Cloudy.
	49	2	0	46,5	54	30,41	81		NE	1	Foggy.
24	29	7	0	29	54	30,39	81		S	1	Fair.
	46	2	0	45	55	30,38	81		S	1	Foggy.
25	31,5	7	0	32	54	30,36	80		E	1	Fair.
	50	2	0	50	56	30,34	80		E	1	Fine.
26	34	7	0	35	54	30,31	84		E	1	Cloudy.
	47	2	0	47	55	30,31	83		E	1	Fair.
27	32,5	7	0	33,5	54	30,41	77		NE	1	Cloudy.
	43	2	0	42	56	30,41	67		E	1	Fine.
28	24,5	7	0	25	53	30,25	83		NE	1	Foggy.
	42	2	0	41,5	56	30,15	75		E	1	Fine.

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for March, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Mar. 1	31	7	o	31	53	30,02	81		ENE	1	Fair.
	40	2	o	39	56	29.98	76		ENE	2	Fine.
2	28	7	o	31	53	29.92	83		E	1	Cloudy.
	44	2	o	44	55	29,90	77		E	1	Fine.
3	32	7	o	33	53,5	29,83	81		E	1	Fair.
	53	2	o	53	57	29,81	69		SSE	1	Fine.
4	35	7	o	35	55	29,72	78		E	1	Fine.
	44	2	o	43,5	55	29,69	74		E	2	Hazy.
5	33	7	o	33	53	29,60	77		E	2	Fair.
	41,5	2	o	41	57	29,54	71		E	2	Fair.
6	36	7	o	35	53	29,71	84		NE	2	Cloudy.
	45	2	o	45	56	29,86	81		WNW	2	Cloudy.
7	33	7	o	33	53	30,15	85		SW	1	Foggy.
	48,5	2	o	48	57	30,19	74		S	1	Fair.
8	36	7	o	36	54	30,02	86	0,335	NE	1	Rain.
	42	2	o	41,5	56	30,03	85		N	1	Cloudy.
9	34	7	o	35	54	30,03	86	0,176	E	1	Cloudy.
	42	2	o	41	55	30,05	78		NE	1	Cloudy.
10	31,5	7	o	33	53	29,86	80		NE	2	Cloudy.
	38,5	2	o	37	55	29,84	74		NE	2	Cloudy.
11	32	7	o	32,5	52	29,78	76		NE	2	Cloudy.
	42	2	o	42	54	29,83	70		NE	2	Fair.
12	32	7	o	32	52	30,00	74		NE	2	Cloudy.
	38	2	o	38	53	30,00	71		NE	2	Fair.
13	31,5	7	o	31,5	51	30,08	75		ENE	2	Cloudy.
	40,5	2	o	40	53	30,12	71		NE	2	Fair.
14	33	7	o	32	51,5	30,11	77		NE	2	Cloudy.
	45	2	o	45	54	30,05	75		NE	2	Cloudy.
15	35,5	7	o	35,5	52	30,03	79		ENE	1	Cloudy.
	47	2	o	46,5	54	30,02	72		E	2	Fine.
16	30,5	7	o	32	52	29,98	83		NE	2	Cloudy.
	47	2	o	47	54	29,97	70		NE	2	Cloudy.

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for March, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Mar. 17	34	7	0	34	53	30,03	75		NE	2	Cloudy.
	42	2	0	42	53	30,00	70		NE	2	Cloudy.
18	31	7	0	33	53	30,16	79		NE	2	Cloudy.
	43	2	0	43	54	30,18	69		NE	2	Cloudy.
19	34	7	0	34	53	30,16	74		NE	1	Cloudy.
	44	2	0	44	54	30,20	70		NE	1	Cloudy.
20	32,5	7	0	35	52	30,20	82		NE	1	Cloudy.
	45	2	0	45	54	30,28	72		NE	1	Cloudy.
21	27,5	7	0	29	52	30,42	75		NE	1	Cloudy.
	43	2	0	43	57	30,42	67		E	1	Fair.
22	27,5	7	0	29,5	52	30,42	75		SE	1	Fair.
	47	2	0	47	56	30,38	64		S b. E	1	Fair.
23	31	7	0	32	52	30,24	73		SSW	1	Fair.
	54	2	0	53	54	30,11	60		SSW	2	Hazy.
24	38	7	0	39	54	30,05	84		SSW	1	Cloudy.
	54	2	0	54	56	29,94	74		SSW	2	Cloudy.
25	45	7	0	45	54	29,66	86		S	2	Cloudy.
	52	2	0	52	56	29,61	75		W	2	Cloudy.
26	38	7	0	42	55	29,82	82	0,076	SW	2	Cloudy.
	49	2	0	48	57	29,70	76		SSW	2	Cloudy.
27	43	7	0	44	55	29,54	84	0,068	SE	1	Cloudy.
	50	2	0	49	56	29,49	80		SE	2	Rain.
28	40	7	0	42	55	29,44	84	0,042	SE	2	Cloudy.
	51	2	0	51	57	29,49	72		SE	2	Cloudy.
29	38	7	0	39	55	29,55	82		E	1	Foggy.
	54	2	0	53,5	59	29,60	72		SW	1	Fair.
30	37	7	0	39	56	29,76	81		SW	1	Cloudy.
	44	2	0	44	57	29,80	82		NE	1	Cloudy.
31	34	7	0	36	56	29,85	83	0,080	SW	1	Fair.
	48	2	0	48	57	29,82	77		SSW	2	Cloudy.

METEOROLOGICAL JOURNAL
for April, 1797.

1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Apr. 1	41	7 0	42	57	29,75	84		S	2	Cloudy.
	52	2 0	51	58	29,70	75		W	2	Cloudy.
2	41	7 0	41	57	29,55	83	0,061	SW	2	Cloudy.
	52	2 0	52	58	29,45	81		W	2	Cloudy.
3	35	7 0	35	56	29,10	84	0,210	NE	1	Rain.
	39	2 0	39	57	29,13	84		NE	1	Rain.
4	34	7 0	37	55	29,23	84	0,520	E	1	Fine.
	52	2 0	50	57	29,40	70		E	1	Hazy.
5	40	7 0	40	55	29,55	85		NE	1	Cloudy.
	44	2 0	44	56	29,53	80		NE	1	Cloudy.
6	36,5	7 0	38	55	29,57	82		NE	1	Cloudy.
	47	2 0	47	56	29,57	76		NE	1	Cloudy.
7	37	7 0	37	55	29,68	83		NE	1	Cloudy.
	48	2 0	48	57	29,75	77		NE	1	Cloudy.
8	40	7 0	40	56	29,97	84		E	1	Cloudy.
	48	2 0	48	57	29,98	81		NE	1	Cloudy.
9	38	7 0	39	55	30,12	83		NE	1	Cloudy.
	50	2 0	50	57	30,11	76		NE	2	Cloudy.
10	40	7 0	42	55	30,07	82		NE	2	Cloudy.
	47	2 0	44	57	29,97	77		NE	2	Cloudy.
11	40	7 0	41	55	29,77	81		NE	2	Cloudy.
	50	2 0	46	57	29,76	79		NE	2	Cloudy.
12	40,5	7 0	42,5	56	29,82	86		NE	1	Cloudy.
	63	2 0	61,5	59	29,80	72		ESE	1	Cloudy.
13	45	7 0	46	58	29,95	84		NE	1	Cloudy.
	54	2 0	54	59	29,89	79		NE	1	Cloudy.
14	42,5	7 0	43	58	29,94	85	0,397	NE	1	Rain.
	48	2 0	48	58	29,93	83		E	1	Cloudy.
15	42,5	7 0	44	58	29,87	87	0,052	E	1	Cloudy.
	56	2 0	55	60	29,83	69		S	1	Cloudy.
16	42	7 0	43	58	29,79	80		NW	1	Cloudy.
	56	2 0	54	60	29,79	73		W	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Apr. 17	38	7	0	40	58	29,77	84		W	1	Fair.
	58	2	0	58	59	29,76	65		W	1	Cloudy.
18	40	7	0	42	57	29,78	70		NW	1	Fair.
	56	2	0	56	59	29,80	63		SW	1	Fair.
19	37	7	0	42	58	29,93	77		NE	1	Fair.
	56	2	0	55	61	29,96	65		NE	1	Fine.
20	36	7	0	40	58	30,13	75		E	1	Fine.
	57	2	0	57	60	30,08	65		E	1	Fine.
21	40	7	0	43	58	29,89	80		E	1	Cloudy.
	59	2	0	59	59	29,83	80		E	1	Cloudy.
22	46	7	0	47	58	29,77	83		E	1	Cloudy.
	61	2	0	61	61	29,78	68		E	1	Fair.
23	45	7	0	47	59	29,94	82		S	1	Fine.
	64	2	0	63	61	29,94	64		S	1	Fair.
24	47	7	0	51	60	29,94	77		SSE	1	Fair.
	65	2	0	65	61	29,94	64		SSW	1	Cloudy.
25	45	7	0	46	60	30,00	82		SW	1	Cloudy.
	62	2	0	61	62	30,03	67		SSW	1	Fair.
26	42	7	0	47	60	30,05	76		SW	1	Fair.
	56	2	0	51	58	29,86	71		SSE	1	Rain.
27	44	7	0	45	59	29,46	82	0,394	NW	2	Cloudy.
	53	2	0	51	60	29,52	78		NW	2	Cloudy.
28	42	7	0	43	59	29,74	79		NE	2	Cloudy.
	55,5	2	0	55	61	29,75	71		NE	1	Fair.
29	42	7	0	47	60	29,65	80		S	1	Cloudy.
	57	2	0	55	61	29,52	76		SSW	1	Rain.
30	44	7	0	44	59	29,38	81	0,225	SSW	1	Cloudy.
	58	2	0	57	60	29,39	67		NW	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	o			o	o	Inches.		Inches.			
May 1	43	7	o	44	58	29.54	77		NW	1	Cloudy.
	57	2	o	54	61	29.60	71		NW	1	Fair.
2	42	7	o	48	59	29.76	84	0,022	S	2	Rain.
	57	2	o	55	59	29.64	80		S	2	Rain.
3	42	7	o	46	59	29.61	82	0,202	SSW	2	Fine.
	58	2	o	56	61	29.62	73		SW	2	Fair.
4	43	7	o	46	59	29.56	83	0,072	S	2	Rain.
	53	2	o	51	61	29.38	84		S	2	Rain.
5	39	7	o	45	58	29.49	76	0,198	SSW	2	Cloudy.
	56	2	o	52	61	29.46	75		S	2	Rain.
6	41	7	o	45	59	29.70	80	0,160	SW	1	Fine.
	57	2	o	55	60	29.79	73		NW	1	Cloudy.
7	42	7	o	44	59	29.91	82	0,196	E	1	Cloudy.
	47	2	o	47	60	29.91	80		NE	1	Cloudy.
8	37	7	o	42	58	30.06	75	0,070	NE	1	Fair.
	52	2	o	52	61	30.03	69		NE	1	Fair.
9	36	7	o	41	58	29.88	79		NE	2	Cloudy.
	49	2	o	48	58	29.77	77		NE	2	Cloudy.
10	34	7	o	40	56	29.61	90	0,265	NE	2	Rain.
	49	2	o	47	58	29.61	84		NE	2	Cloudy.
11	41,5	7	o	42	56	29.58	85		NE	1	Cloudy.
	50	2	o	50	57	29.55	77		NE	1	Cloudy.
12	42	7	o	45	57	29.58	82		E	1	Cloudy.
	60	2	o	59	59	29.58	70		E	1	Fair.
13	42	7	o	46	58	29.88	78	0,068	SSW	2	Fair.
	60	2	o	59	61	29.98	65		S	2	Fair.
14	42	7	o	47	58	30.17	73		NW	1	Fair.
	61	2	o	61	60	30.19	66		E	1	Hazy.
15	43	7	o	47	59	30.08	80		E	1	Cloudy.
	62	2	o	61,5	61	30.00	71		E	1	Fine.
16	47	7	o	53	60	29.91	81		E	1	Fair.
	67	2	o	67	62	29.85	74		E	1	Fair.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
May 17	53	7	0	56	61	29,86	76		S	2	Cloudy.
	63	2	0	61	62	29,80	83		S	2	Rain.
18	52	7	0	54	62	29,91	83	0,115	S	2	Cloudy.
	68	2	0	67	62	29,96	77		SSW	2	Cloudy.
19	51	7	0	55	62	29,99	81		S	1	Cloudy.
	73	2	0	71	65	29,90	72		ESE	2	Fair.
20	59	7	0	61	64	29,67	76		SSW	2	Cloudy.
	68	2	0	68	65	29,77	67		SSW	2	Fair.
21	52	7	0	54	63	29,91	76		SSW	2	Fair.
	68	2	0	68	64	29,95	66		SSW	2	Fine.
22	49	7	0	52	63	30,08	75		SSW	2	Fair.
	66	2	0	66	64	30,17	61		SW	1	Fine.
23	48	7	0	53	63	30,33	72		W	1	Hazy.
	69	2	0	68	64	30,33	65		SE	1	Fair.
24	48	7	0	54	64	30,28	75		E	1	Fair.
	71	2	0	70	65	30,20	63		E	2	Fine.
25	50	7	0	58	64	30,08	73		E	2	Fair.
	79	2	0	78	68	30,03	68		SSE	2	Fine.
26	61	7	0	64	68	29,95	76		W	1	Fair.
	73	2	0	73	68	29,96	66		SSW	1	Cloudy.
27	50	7	0	54	66	30,09	75		SSW	2	Fair.
	71	2	0	71	67	30,09	65		SW	2	Fair.
28	50	7	0	54	65	29,98	76		S	1	Cloudy.
	70	2	0	70	66	29,83	66		SSE	2	Fair.
29	51	7	0	54	65	29,70	74		SSW	2	Fair.
	62	2	0	62	64	29,59	76		S	2	Rain.
30	49	7	0	50	63	29,77	81	0,068	NE	2	Cloudy.
	60	2	0	59	62	29,99	72		NE	2	Cloudy.
31	46	7	0	53	62	30,10	74		SSW	2	Hazy.
	65	2	0	65	62	30,00	69		SW	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
June 1	52	7 0	54	62	29,71	83		SW	2	Cloudy.
	65	2 0	64	62	29,67	75		SW	2	Cloudy.
2	49	7 0	53	62	29,78	71	0,295	E	1	Cloudy.
	69	2 0	69	63	29,76	66		S	1	Cloudy.
3	51	7 0	54	62	29,36	85	0,570	SSW	1	Rain.
	56	2 0	48	61	29,52	80		WNW	1	Rain.
4	40	7 0	45	60	29,82	73	0,225	W	2	Fine.
	63	2 0	63	60	29,90	66		W	2	Cloudy.
5	46	7 0	50	60	30,04	73		NE	1	Fair.
	63	2 0	62	61	30,14	64		NE	1	Fine.
6	43	7 0	46	60	30,29	76		NE	1	Fine.
	64	2 0	63	61	30,27	65		NE	1	Fine.
7	45	7 0	53	60	30,01	78		W	1	Cloudy.
	62	2 0	58	61	29,89	80		NW	1	Rain.
8	45	7 0	50	60	29,78	75		SW	1	Fine.
	64	2 0	63	60	29,71	65		SW	1	Fair.
9	46	7 0	49	59	29,75	78	0,210	NE	1	Cloudy.
	61	2 0	60	61	29,84	70		E	1	Cloudy.
10	47	7 0	53	59	30,06	77	0,060	NE	2	Fair.
	62	2 0	60	60	30,18	70		NE	2	Fair.
11	44	7 0	52	60	29,98	73		N	1	Fine.
	69	2 0	66	60	29,88	66		N	1	Cloudy.
12	53	7 0	55	60	29,66	81	0,050	SW	1	Fair.
	65	2 0	60	61	29,66	77		W	1	Rain.
13	49	7 0	53	61	29,68	80	0,218	E	1	Cloudy.
	65	2 0	65	62	29,68	68		E	1	Fair.
14	50	7 0	54	61	29,74	78		W	1	Cloudy.
	69	2 0	68	62	29,74	70		W	1	Fair.
15	51	7 0	53	62	29,76	82	0,045	W	1	Cloudy.
	64	2 0	64	62	29,80	79		N	1	Cloudy.
16	50	7 0	53	62	30,04	78	0,185	NE	1	Cloudy.
	66	2 0	65	62	30,08	69		NE	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
June 17	53	7 0	55	62	30,16	75		S	1	Fair.
	71	2 0	69	63	30,16	70		SW	1	Cloudy.
18	51	7 0	57	62	30,15	75		E	1	Fine.
	73	2 0	73	63	30,10	67		E	1	Fair.
19	49	7 0	56	62	29,94	76		E	1	Fine.
	71	2 0	70	63	29,84	66		E	1	Cloudy.
20	52	7 0	56	63	29,77	80		E	1	Fair.
	71	2 0	68	65	29,71	70		ESE	1	Cloudy.
21	51	7 0	54	63	29,68	74	0,673	E	1	Cloudy.
	65	2 0	63	64	29,64	74		E	1	Cloudy.
22	53	7 0	55	63	29,54	81	0,088	E	1	Cloudy.
	67	2 0	63	63	29,44	69		ESE	2	Cloudy.
23	51	7 0	53	63	29,34	84	0,280	ESE	1	Rain.
	63	2 0	62	63	29,39	81		W	1	Cloudy.
24	52	7 0	54	62	29,75	82	0,535	SW	1	Cloudy.
	65	2 0	65	63	29,84	76		WSW	1	Cloudy.
25	52	7 0	53	62	30,03	83	0,570	SW	1	Rain.
	69	2 0	68	63	30,04	66		SW	1	Fair.
26	50	7 0	53	62	30,08	80		SW	1	Fair.
	66	2 0	65	64	30,07	68		SW	1	Fair.
27	50	7 0	53	62	30,02	81	0,022	N	1	Cloudy.
	67	2 0	65	64	29,93	71		NE	1	Cloudy.
28	51	7 0	54	62	29,99	79		E	1	Cloudy.
	70	2 0	70	64	30,00	65		SE	1	Fair.
29	50	7 0	53	63	29,87	82	0,135	W	1	Rain.
	66	2 0	64	63	29,82	67		NE	1	Fair.
30	50	7 0	54	63	30,06	77	0,062	NE	1	Cloudy.
	67	2 0	64	63	30,10	69		SE	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
July 1	54	7	0	56	63	30,07	78		SW	1	Cloudy.
	70	2	0	68	63	30,01	74		S	1	Cloudy.
2	51	7	0	56	63	30,00	81		SW	1	Cloudy.
	72	2	0	69	64	30,03	71		SW	2	Fair.
3	54	7	0	55	63	29,91	80		SW	2	Cloudy.
	64	2	0	61	63	29,74	80		S	2	Cloudy.
4	48	7	0	55	63	29,91	78	0,101	W	2	Fair.
	69	2	0	67	64	29,94	69		W	2	Cloudy.
5	54	7	0	55	63	29,76	80	0,020	SSW	2	Rain.
	65	2	0	63	63	29,64	83		SSW	2	Cloudy.
6	51	7	0	56	62	29,51	79	0,063	SSW	2	Cloudy.
	66	2	0	64	63	29,51	74		WNW	2	Cloudy.
7	52	7	0	57	62	29,72	79		W	2	Cloudy.
	68	2	0	65	62	29,76	71		W	1	Cloudy.
8	54	7	0	57	63	29,77	80		NW	1	Cloudy.
	67	2	0	66	63	29,88	75		NW	1	Cloudy.
9	54	7	0	58	63	30,14	78		NE	1	Fine.
	75	2	0	74	64	30,19	68		N	1	Cloudy.
10	58	7	0	60	64	30,25	82		SW	1	Cloudy.
	81	2	0	78	66	30,24	72		S	1	Fair.
11	59	7	0	61	66	30,13	80	0,082	S	1	Fine.
	80	2	0	79	68	30,03	67		S	1	Fine.
12	57	7	0	59	66	29,92	78		S	2	Cloudy.
	74	2	0	73	67	29,92	70		S	2	Cloudy.
13	55	7	0	58	66	29,92	77		SE	1	Fair.
	76	2	0	75	68	29,88	69		SE	1	Fine.
14	61	7	0	66	68	29,84	82	0,110	E	1	Fine.
	85	2	0	84	69	29,87	70		SE	1	Fine.
15	60	7	0	66	69	29,90	74		E	1	Cloudy.
	82	2	0	81	71	29,92	68		E	1	Hazy.
16	59	7	0	64	70	30,04	79		SW	1	Fine.
	85	2	0	84	73	29,99	67		SSW	1	Fair.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
July 17	62	7	0	65	71	29,95	77	0,420	S	1	Fine. [Much thunder and lightning last night.
	84	2	0	83	73	29,96	66		S	1	Fine.
18	56	7	0	61	71	30,07	75		S	1	Fine.
	76	2	0	76	72	30,07	64		SW	1	Fair.
19	57	7	0	60	70	30,07	77		SSW	2	Cloudy.
	73	2	0	73	70	30,02	68		SSW	2	Fair.
20	59	7	0	59	69	30,00	81	0,125	S	1	Rain.
	71	2	0	69	69	29,98	76		SSW	1	Fair.
21	51	7	0	56	68	30,11	78	0,102	SW	1	Fair.
	71	2	0	71	69	30,14	66		SW	1	Fair.
22	51	7	0	55	66	30,16	77		W	1	Cloudy.
	63	2	0	63	66	30,15	75		W	1	Cloudy.
23	50	7	0	56	64	30,21	81		W	1	Fine.
	75	2	0	75	68	30,23	66		W	1	Fair.
24	55	7	0	63	66	30,20	78		W	1	Cloudy.
	80	2	0	79	69	30,13	66		W	1	Fine.
25	56	7	0	59	67	30,11	82		SW	1	Cloudy.
	80	2	0	79	73	30,10	71		W	1	Fine.
26	60	7	0	63	69	30,07	80		SW	1	Fine.
	85	2	0	83	73	30,04	66		SSW	1	Fine.
27	63	7	0	66	71	29,98	75		W	1	Hazy.
	80	2	0	80	74	29,99	68		W	1	Fair.
28	61	7	0	64	69	30,04	74		N	1	Hazy.
	75	2	0	74	72	30,04	68		SSE	1	Hazy.
29	58	7	0	65	71	29,93	80		E	1	Cloudy.
	78	2	0	76	73	29,86	70		E	1	Fair.
30	62	7	0	62	72	29,61	87	0,265	NE	1	Rain.
	76	2	0	75	72	29,69	72		S	2	Fair.
31	59	7	0	62	68	29,74	77		S	2	Cloudy.
	73	2	0	73	70	29,74	69		S	2	Fair.

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1797	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Aug. 1	58	7	0	58	69	29,72	84	0,123	SE	1	Rain.
	72	2	0	70	69	29,60	76		S	2	Cloudy.
2	53	7	0	57	68	29,76	80	0,142	SSW	1	Fine.
	70	2	0	65	69	29,74	74		SSW	1	Rain.
3	54	7	0	58	67	29,74	77	0,190	SSW	2	Cloudy.
	69	2	0	66	67	29,74	78		SW	2	Rain.
4	53	7	0	54	66	29,81	81	0,133	SSW	2	Cloudy.
	71	2	0	69,5	67	29,85	68		W	1	Fair.
5	55	7	0	57	67	29,97	79	0,080	W	1	Fair.
	71	2	0	65	67	29,99	71		W	1	Rain.
6	50	7	0	53	66	30,09	83	0,083	SSW	1	Fine.
	70	2	0	68	67	30,10	73		SSW	1	Fair.
7	51	7	0	55	65	30,09	78	0,038	SSW	1	Fine.
	70	2	0	70	66	30,09	66		SSW	1	Cloudy.
8	55	7	0	58	65	29,93	78		ESE	1	Fair.
	76	2	0	76	66	29,79	67		SSE	2	Fine.
9	55	7	0	57	67	29,74	73		SW	2	Fair.
	71	2	0	69	67	29,81	68		SW	2	Cloudy.
10	54	7	0	57	66	29,93	77	0,030	SW	1	Cloudy.
	69,5	2	0	68	66	29,94	70		SW	1	Cloudy.
11	58	7	0	58	66	29,72	80		SSW	1	Rain.
	64	2	0	63	66	29,68	80		S	2	Cloudy.
12	50	7	0	53	65	29,78	80	0,057	S	1	Fine.
	72	2	0	71	67	29,78	68		SW	1	Cloudy.
13	49	7	0	53	65	29,78	78		SW	1	Cloudy.
	70	2	0	69	65	29,81	66		SW	2	Cloudy.
14	53	7	0	56	65	29,90	82	0,113	SW	1	Fine.
	73	2	0	73	66	29,98	67		SW	1	Fair.
15	53	7	0	57	65	29,96	75		S	1	Cloudy.
	71	2	0	69	65	29,88	73		S	2	Cloudy.
16	56	7	0	57	66	29,77	83	0,110	NE	1	Cloudy.
	68	2	0	68	65	29,83	75		S	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Aug. 17	51	7 0	55	65	29,97	80		S	1	Fine.
	72	2 0	71	67	29,91	70		SE	1	Fair.
18	58	7 0	58	66	29,48	88	0,195	E	1	Cloudy.
	72	2 0	69	66	29,60	69		S	2	Fair.
19	55	7 0	58	67	29,66	82		SSE	1	Fair.
	70	2 0	70	67	29,64	68		S	1	Fair.
20	53	7 0	58	66	29,85	81	0,170	SW	1	Cloudy.
	66	2 0	61	66	29,84	80		S	1	Rain.
21	54	7 0	56	65	29,86	80	0,156	SW	2	Fair.
	70	2 0	69	68	29,87	69		WSW	2	Fine.
22	48	7 0	53	63	30,07	78		WSW	1	Fine.
	67	2 0	67	66	30,10	67		NW	1	Cloudy.
23	50	7 0	52	64	30,14	81		NW	1	Cloudy.
	67	2 0	67	65	30,14	73		NW	1	Cloudy.
24	52	7 0	56	64	30,18	81	0,025	W	1	Cloudy.
	71	2 0	70	65	30,18	70		SW	1	Cloudy.
25	56	7 0	58	64	30,04	83	0,074	SE	1	Cloudy.
	65	2 0	64	65	30,01	78		S	1	Cloudy.
26	48	7 0	52	64	30,02	82		S	1	Fine.
	71	2 0	71	66	29,95	73		S	1	Fair.
27	60	7 0	61	65	29,83	86	0,150	S	1	Rain.
	72	2 0	72	67	29,83	75		S	2	Fair.
28	59	7 0	61	66	29,80	86	0,095	S	2	Rain.
	70	2 0	70	66	29,80	81		S	2	Cloudy.
29	57	7 0	62	66	29,88	80		S	2	Cloudy.
	72	2 0	71	67	29,81	78		S	2	Cloudy.
30	59	7 0	59	67	29,70	86	0,825	S	1	Fair.
	69	2 0	69	67	29,77	70		S	1	Fair.
31	52	7 0	57	66	29,90	82		SSE	1	Cloudy.
	70	2 0	70	66	29,86	71		S	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 1	57	7	0	60	66	29,53	86	0,075	E	1	Cloudy.
	71	2	0	69	67	29,45	78		SE	2	Cloudy.
2	53	7	0	53	66	29,61	84	0,840	WNW	1	Rain.
	62	2	0	62	66	29,80	75		WNW	1	Cloudy.
3	44	7	0	47	63	30,10	80		SW	1	Fine.
	64	2	0	64	64	30,13	67		NW	1	Cloudy.
4	45	7	0	52	64	30,13	77		E	1	Cloudy.
	64	2	0	64	64	30,11	67		SW	1	Cloudy.
5	44	7	0	46	64	30,14	80		W	1	Fine.
	68	2	0	68	64	30,07	67		W	1	Fair.
6	54	7	0	55	62	29,83	81	0,112	S	2	Rain.
	68	2	0	68	63	29,79	78		SW	2	Cloudy.
7	55	7	0	55	63	29,74	89	0,550	S	2	Cloudy.
	69	2	0	69	64	29,70	70		SW	2	Fair.
8	51	7	0	52	64	29,84	77		W	1	Cloudy.
	63	2	0	63	64	29,84	66		SSW	1	Fair.
9	47	7	0	49	63	29,67	77	0,145	SW	2	Fine.
	62	2	0	62	63	29,84	65		W	2	Fair.
10	42	7	0	45	62	30,07	78		W	1	Fine.
	62	2	0	62	63	29,98	70		W	1	Fair.
11	50	7	0	51	62	29,37	80		E	1	Rain.
	59	2	0	58	62	29,04	90		E	1	Rain.
12	50	7	0	51	61	29,51	81	0,505	SSW	2	Fine.
	60	2	0	58	61	29,57	73		SW	2	Fair.
13	52	7	0	53	61	29,80	83	0,030	SSW	2	Fine.
	66	2	0	64	63	29,73	71		S	2	Fair.
14	55	7	0	57	62	29,61	82	0,275	S	2	Cloudy.
	62	2	0	62	62	29,54	78		S	2	Cloudy.
15	48	7	0	50	61	29,78	81	0,088	S	1	Fine.
	64	2	0	64	61	29,96	75		W	1	Fair.
16	47	7	0	49	61	30,13	82		SSW	1	Fair.
	64	2	0	64	61	30,05	73		S	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 17	55	7	0	58	61	29,67	90	0,203	SW	2	Cloudy.
	68	2	0	68	62	29,73	78		SW	1	Cloudy.
18	51	7	0	52	61	29,62	82	0,050	SSW	1	Fine.
	64	2	0	64	62	29,62	74		SSW	1	Fair.
19	49	7	0	52	61	29,61	82	0,032	E	1	Cloudy.
	60	2	0	57	61	29,43	81		E	1	Rain.
20	49	7	0	51	60	29,41	83	0,390	S	2	Cloudy.
	59	2	0	57	61	29,43	76		S	2	Fair.
21	46	7	0	47	60	29,72	85		S	1	Fine.
	63	2	0	63	62	29,79	70		S	1	Fine.
22	42	7	0	43	60	29,89	84		E	1	Hazy.
	61	2	0	61	61	29,91	73		E	1	Cloudy.
23	50	7	0	51	60	29,80	88		NE	1	Cloudy.
	65	2	0	65	62	29,71	75		E	1	Fine.
24	55	7	0	55	61	29,60	87		SW	1	Cloudy.
	62	2	0	62	62	29,57	80		E	1	Cloudy.
25	53	7	0	53	61	29,55	88	0,270	N	1	Cloudy.
	60	2	0	60	62	29,64	87		N	1	Rain.
26	55	7	0	55	61	29,75	90	0,410	NE	1	Cloudy.
	63	2	0	62	62	29,71	84		NE	1	Cloudy.
27	53	7	0	53	61	29,69	86		ESE	1	Cloudy.
	63	2	0	62	62	29,73	79		E	1	Cloudy.
28	52	7	0	54	61	29,90	87		NE	1	Foggy.
	64	2	0	64	62	29,94	81		NE	1	Hazy.
29	54	7	0	55	62	29,96	88		E	1	Cloudy.
	65	2	0	64	62	29,86	79		S	1	Cloudy.
30	53	7	0	54	61	29,54	88	0,085	S	2	Rain.
	61	2	0	60	61	29,58	72		S	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Oct. 1	50	7	0	52	61	29,85	84		SW	1	Cloudy.
	57	2	0	57	61	29,96	75		W	1	Fair.
2	46	7	0	46	61	30,02	84		E	1	Fair.
	62	2	0	62	61	30,00	77		S	1	Cloudy.
3	53	7	0	54	61	29,99	86	0,063	E	1	Fair.
	63	2	0	62	61	29,96	75		SE	1	Fair.
4	53	7	0	53	61	30,04	86		NE	1	Cloudy.
	61	2	0	61	62	30,08	77		NE	1	Fair.
5	50	7	0	51	61	30,12	86		NE	1	Cloudy.
	59	2	0	59	60	30,09	82		NE	1	Cloudy.
6	52	7	0	52	61	29,97	87	0,065	NE	1	Cloudy.
	57	2	0	57	60	29,91	84		NE	1	Rain.
7	44	7	0	45	60	29,93	83		NE	1	Fine.
	58	2	0	57	60	29,95	82		NE	1	Fair.
8	44	7	0	46	59	30,00	84		W	1	Cloudy.
	61	2	0	60	60	29,96	82		S	2	Cloudy.
9	51	7	0	52	59	30,02	87		WSW	1	Cloudy.
	61	2	0	60	60	30,07	73		NNE	1	Fair.
10	49	7	0	50	59	30,27	73		NE	1	Cloudy.
	58	2	0	58	58	30,28	67		NE	1	Cloudy.
11	50	7	0	50	58	30,31	80		NW	1	Cloudy.
	57	2	0	57	58	30,31	84		NW	1	Cloudy.
12	46	7	0	49	58	30,31	83		NW	1	Cloudy.
	57	2	0	57	58	30,25	78		NW	1	Cloudy.
13	48	7	0	48	58	30,25	82		NW	1	Fair.
	57	2	0	57	58	30,14	72		NW	1	Fair.
14	46	7	0	47	59	29,93	81	0,035	NW	1	Fine.
	53	2	0	52	57	30,08	73		NW	1	Fine.
15	39	7	0	42	57	30,19	81		SW	1	Cloudy.
	58	2	0	57	57	30,16	79		SW	1	Cloudy.
16	48	7	0	51	57	30,14	85		S	1	Cloudy.
	58	2	0	58	58	30,12	78		SSW	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Oct. 17	50	7	0	53	57	30,03	85		SW	1	Cloudy.
	60	2	0	59	58	29,99	88		SW	1	Cloudy.
18	50	7	0	53	57	29,57	83		S	2	Cloudy.
	56	2	0	55	59	29,57	70		W	2	Fair.
19	39	7	0	40	56	29,62	79		SW	1	Fair.
	50	2	0	49	58	29,63	74		SW	1	Fine.
20	42	7	0	43	57	29,37	89	0,385	NE	1	Cloudy.
	46	2	0	45	57	29,30	83		NE	1	Cloudy.
21	36	7	0	36	56	29,38	83		SW	1	Fine.
	48	2	0	48	57	29,40	71		SW	1	Fine.
22	35	7	0	36	56	29,48	82		SSW	1	Cloudy.
	48	2	0	48	57	29,51	80		SSE	2	Cloudy.
23	42	7	0	44	55	29,53	86	0,145	E	1	Rain.
	45	2	0	44	55	29,54	87		ESE	1	Rain.
24	42	7	0	44	54	29,45	90	0,923	E	1	Cloudy.
	51	2	0	51	56	29,33	83		E	1	Cloudy.
25	42	7	0	42	55	29,12	87	0,050	SE	1	Rain.
	48	2	0	48	56	29,05	80		WSW	1	Cloudy.
26	38	7	0	39	55	29,10	86	0,240	N	1	Cloudy.
	46	2	0	45	56	29,38	83		W	1	Cloudy.
27	35	7	0	37	54	29,76	87		N	1	Fair.
	46	2	0	46	55	29,80	83		NE	1	Cloudy.
28	35	7	0	33	54	29,96	85		NE	1	Fair.
	47	2	0	47	54	29,94	79		NE	2	Cloudy.
29	36	7	0	42	53	29,73	81		NE	2	Cloudy.
	42	2	0	42	54	29,57	85		NE	1	Rain.
30	41	7	0	42	53	29,60	85	0,095	NE	2	Cloudy.
	46	2	0	46	54	29,63	83		NE	2	Cloudy.
31	41	7	0	42	53	29,77	81		NE	2	Cloudy.
	46	2	0	46	54	29,90	75				

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Nov. 1	34	7 0	35	52	30,18	81		NE	2	Fine.
	47	2 0	47	54	30,27	73		NE	2	Fair.
2	31	7 0	32	52	30,23	82		W	1	Cloudy.
	44	2 0	44	53	30,09	85		SW	1	Cloudy.
3	42	7 0	44	53	29,89	88		SW	2	Cloudy.
	52	2 0	52	55	29,84	81		SW	1	Cloudy.
4	44	7 0	44	54	29,70	83		SW	1	Cloudy.
	48	2 0	48	56	29,57	83		E	1	Cloudy.
5	44	7 0	45	54	29,41	87	0,102	E	1	Rain.
	55	2 0	55	58	29,43	89		S	2	Cloudy.
6	50	7 0	50	55	29,88	89		SE	1	Fair.
	57	2 0	57	59	29,96	86		E	1	Fine.
7	49	7 0	48	57	30,11	91		E	1	Foggy.
	55	2 0	55	59	30,17	89		E	1	Fair.
8	47	7 0	47	58	30,39	91		E	1	Foggy.
	54	2 0	53	59	30,41	89		E	1	Fair.
9	41	7 0	42	57	30,48	90		E	1	Foggy.
	48	2 0	47	58	30,46	89		E	1	Cloudy.
10	44	7 0	44	57	30,39	88		E	1	Cloudy.
	52	2 0	50	57	30,39	82		E	1	Fair.
11	40	7 0	40	56	30,38	88		E	1	Fine.
	52	2 0	52	59	30,35	80		E	1	Fair.
12	40	7 0	40	56	30,24	88				Foggy.
	45	2 0	45	57	30,20	88		W	1	Foggy.
13	34	7 0	34	56	30,11	86		NE	1	Foggy.
	51	2 0	51	58	30,07	87		NE	1	Fair.
14	45	7 0	50	57	30,04	90		S	2	Cloudy.
	53	2 0	53	58	30,11	81		SW	2	Cloudy.
15	43	7 0	44	57	30,09	88	0,102	SW	1	Fine.
	51	2 0	51	59	30,15	78		WSW	1	Fair.
16	41	7 0	42	57	30,15	84		WSW	1	Cloudy.
	52	2 0	52	58	30,16	83		NE	1	Cloudy.

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	°			°	°	Inches.		Inches.			
Nov. 17	35	7	0	35	57	30,32	85		NE	1	Fair.
	44	2	0	44	57	30,28	79		NE	1	Fine.
18	33	7	0	37	56	29,86	84	0,060	SW	1	Rain.
	46	2	0	44	57	29,76	84		NE	1	Cloudy.
19	31	7	0	32	53	29,88	84		NE	2	Snow.
	37	2	0	37	55	29,96	81		NE	2	Fair.
20	27	7	0	30	53	29,97	83		W	1	Cloudy.
	43	2	0	38	54	29,77	85		SSW	1	Rain.
21	34	7	0	35	52	29,73	83	0,060	NW	1	Fine.
	42	2	0	40	53	29,71	79		NW	1	Fine.
22	38	7	0	39	52	29,22	80		WNW	2	Cloudy.
	45	2	0	45	53	29,14	73		WNW	2	Fair.
23	30	7	0	32	51	29,43	86		SW	1	Cloudy.
	36	2	0	36	51	29,47	84		SSW	1	Cloudy.
24	27	7	0	27	49	29,64	85		SSW	1	Fair.
	36	2	0	36	52	29,68	80		N	1	Cloudy.
25	28	7	0	30	50	29,78	82		NE	1	Cloudy.
	45	2	0	39	52	29,78	80		E	1	Cloudy.
26	44	7	0	53	52	29,62	91	0,380	S	2	Cloudy.
	55	2	0	55	54	29,66	90		S	2	Cloudy.
27	49	7	0	50	54	29,74	88	0,102	SW	2	Fair.
	52	2	0	52	56	29,83	81		SW	1	Hazy.
28	47	7	0	47	54	29,82	90	0,020	SW	1	Rain.
	47	2	0	44	56	29,78	91		NE	1	Rain.
29	35	7	0	35	53	29,78	88	0,502	NE	1	Rain.
	48	2	0	37	53	29,68	87		NE	1	Cloudy.
30	36	7	0	40	52	29,44	90	0,145	SW	1	Cloudy.
	49	2	0	49	56	29,46	90		SW	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Dec. 1	45	8	0	48	54	29.35	91	0,058	S	2	Cloudy.
	51	2	0	48	57	29.27	88		S	2	Rain.
2	41	8	0	41	53	29.20	80		S	2	Fine.
	47	2	0	47	55	29.23	78		SW	2	Fair.
3	31	8	0	31	53	29.60	83		WSW	1	Fair.
	38	2	0	38	54	29.78	78		NW	1	Cloudy.
4	29	8	0	30	51	30.02	84		NE	1	Fair.
	52	2	0	42	53	29.85	84		E	1	Cloudy.
5	42	8	0	45	53	29.87	88		WSW	1	Fair.
	54	2	0	54	55	30.01	83		SW	1	Fair.
6	50	8	0	50	55	29.65	88	0,315	W	1	Rain.
	48	2	0	45	57	29.85	70		WNW	1	Fine.
7	33	8	0	35	53	30.16	82		WNW	1	Fair.
	41	2	0	41	57	30.15	76		WNW	1	Cloudy.
8	34	8	0	36	53	30.02	78		NW	1	Cloudy.
	50	2	0	45	54	29.79	80		SW	2	Cloudy.
9	37	8	0	37	51	29.62	84	0,110	SW	1	Fair.
	43	2	0	42	56	29.67	80		S	1	Fine.
10	36	8	0	38	53	29.34	86	0,020	SSW	1	Rain.
	40	2	0	40	55	29.33	77		WSW	1	Fair.
11	31	8	0	33	51	29.53	85		WNW	1	Cloudy.
	34	2	0	34	53	29.65	85		NW	1	Fair.
12	31	8	0	40	50	29.36	84		S	2	Rain.
	43	2	0	43	52	29.11	89		S	2	Rain.
13	34	8	0	35	49	29.33	88	0,075	S	1	Fine.
	44	2	0	43	53	29.28	88		S	2	Rain.
14	38	8	0	38	52	29.07	89	0,625	S	1	Fair.
	44	2	0	44	54	29.12	86		S	1	Fair.
15	37	8	0	38	53	29.50	86		SSW	1	Cloudy.
	51	2	0	47	54	29.44	89		S	2	Rain.
16	48	8	0	49	54	29.50	89	0,105	S	2	Rain.
	53	2	0	53	57	29.48	87		S	2	Cloudy.

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	o			o	o	Inches.		Inches.			
Dec. 17	53	8	o	53	56	29,28	85		S	3	Cloudy.
	50	2	o	48	58	29,44	82		SSW	2	Rain.
18	44	8	o	46	56	29,88	88	0,020	S	2	Cloudy.
	52	2	o	52	58	29,84	85		S	2	Cloudy.
19	50	8	o	52	56	29,98	83		SE	2	Cloudy.
	56	2	o	56	58	30,02	83		SSE	2	Fair.
20	51	8	o	51	57	30,17	88		SW	1	Cloudy.
	54	2	o	54	60	30,22	87		W	1	Cloudy.
21	34	8	o	36	57	30,32	85		NE	1	Foggy.
	45	2	o	44	57	30,24	84		E	1	Cloudy.
22	43	8	o	44	57	30,04	89	0,210	SE	1	Rain.
	49	2	o	49	59	29,95	89		SE	1	Cloudy.
23	40	8	o	40	54	29,87	89				Foggy.
	40	2	o	39	58	29,88	88		NE	1	Cloudy.
24	35	8	o	35	56	30,18	88		SW	1	Cloudy.
	37	2	o	37	57	30,24	88		WNW	1	Cloudy.
25	32	8	o	32	54	30,45	87		W	1	Fair.
	38	2	o	38	57	30,45	86		WSW	1	Fair.
26	33	8	o	47	55	30,34	91	0,068	SW	1	Cloudy.
	50	2	o	50	57	30,33	91		SW	1	Cloudy.
27	47	8	o	47	55	30,32	89		SW	1	Cloudy.
	50	2	o	50	58	30,32	87		WSW	1	Cloudy.
28	31	8	o	33	54	30,46	86		SW	1	Fair.
	42	2	o	41	56	30,43	81		WNW	1	Fair.
29	40	8	o	42	54	30,20	85		W	1	Cloudy.
	47	2	o	46	56	29,95	84		W	1	Cloudy.
30	41	8	o	41	53	29,80	76	0,005	W	2	Fair.
	46	2	o	44	56	29,93	75		NW	1	Fair.
31	38	8	o	44	54	29,63	78		WNW	1	Cloudy.
	47	2	o	46	55	29,62	76		W	1	Cloudy.

1797.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	49	25	37,3	49	25	37,7	56	45	51,2	30,50	29,52	30,09	90	69	85,1	0,960
February	50	24,5	37,5	50	25	37,9	57	50	53,3	30,62	29,37	30,31	88	67	81,1	0,219
March	54	27,5	39,9	54	29	40,2	59	51	54,3	30,42	29,44	29,94	86	60	76,6	0,777
April	65	34	47,4	65	35	47,8	62	55	57,8	30,13	29,10	29,77	87	63	77,3	1,859
May	79	34	53,8	78	40	55,4	68	56	61,5	30,33	29,38	29,89	90	61	75,1	1,436
June	73	40	57,5	73	45	58,6	65	59	61,8	30,29	29,36	29,86	85	64	74,3	4,223
July	85	48	65,8	84	55	66,7	74	62	67,4	30,25	29,51	29,96	83	64	74,6	1,288
August	76	48	61,9	76	52	62,6	69	63	66,1	30,18	29,48	29,87	88	66	76,4	2,789
September	71	42	56,9	69	45	57,5	67	60	62,2	30,14	29,04	29,75	90	65	79,3	4,061
October	63	35	49,0	62	35	49,6	62	53	57,5	30,31	29,05	29,83	90	67	81,3	2,001
November	57	27	45,3	57	27	43,4	59	49	55,0	30,48	29,14	29,92	91	73	85,0	1,473
December	56	29	42,7	56	30	43,0	60	49	54,9	30,46	29,07	29,80	91	70	84,5	1,611
Whole year			49,4			50,0			58,6			29,92			79,1	22,697

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

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